FISCAL YEAR 1989 DEPARTMENT OF ENERGY AUTHORIZATION

(Magnetic Fusion Energy)

HEARING

BEFORE THE

SUBCOMMITTEE ON
ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON
SCIENCE, SPACE, AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

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1989 DEPARTMENT OF ENERGY AUTHORIZATION

WEDNESDAY, MARCH 30, 1988

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY,
SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT,
Washington, DC.

The Subcommittee met, pursuant to notice, at 1:05 p.m., in room 2318, Rayburn House Office Building, Hon. Marilyn Lloyd (Chairman of the Subcommittee) presiding.

Mrs. LLOYD. Good afternoon. The Subcommittee on Energy Research and Development will come to order, and it is good to see some of you back from lunch here today. I realize that it is a pretty day outside and it is a temptation to walk outside a little while. But it is good to see you that are here and your interest in the fusion program, and it is good to see John Clarke here with Dr. Decker again. I understand this is our tenth anniversary that you have been appearing before this committee, and we have certainly enjoyed working with you. Your expertise and your commitment to the energy program for our nation are appreciated here very much.

I think it is best to put our hearings in the context of the overall deliberations in the FY 1989 budget. As you know, the Budget Committee has placed ceilings on the Energy Function 270, which allows some growth for our energy R&D programs. We have nevertheless a very tight squeeze on the Department of Energy's R&D activities, since basic energy sciences, nuclear fission, and magnetic fusion need significant growth. The Administration has also, as in previous years, greatly reduced or eliminated technology development programs that this Subcommittee and the Congress has strongly supported, and I believe will be compelled to restore.

Despite these budget pressures, we do remain committed to support activities such as the magnetic fusion energy program to assure that future generations have adequate sources at reasonable prices.

The Department of Energy is requesting $360 million in 1989 to support its core program of theoretical and experimental plasma physics and the development and operation of several fusion devices. These include new initiatives, such as the Compact Ignition Tokamak (CIT) and the International Thermonuclear Experimental Reactor (ITER).

We are particularly interested in the last two mentioned initiatives. I understand that the CIT project has recently undergone a conceptual design review by a non-DOE, non-Princeton team. Ap-

parently the baseline CIT design has been enhanced and we are interested in learning of the recommendations resulting from that review and from the implications of the design changes.

I am still concerned about the U.S. involvement in the ITER project. It is not that we are against international collaboration—in fact, we warmly endorse the exchange of scientists, hardware, and data that have occurred in the past. However, I do worry that too much reliance on ITER will cause the United States to slacken in its drive to achieve its fundamental program objectives. The strength of the domestic program must be our primary concern, and so I wonder if we cannot utilize these foreign programs to finish gathering information in some disciplines while the United States program moves ahead into promising new regimes, such as with CIT. Perhaps these hearings can give some insight into moderating this approach.

I now yield to my good friend and colleague, the Ranking Republican member, Mr. Sid Morrison, for his opening remarks.

Mr. MORRISON. Thank you, Madam Chairman, and good afternoon. We have made some good progress in understanding the issues involved in fusion energy development and recently have had the help of an excellent report prepared by the Office of Technology Assessment.

I was impressed with the breakthroughs in the fusion program over the last couple of years, the ability to achieve a world record beta, which is the ratio of plasma energy to magnetic energy. It's something that we can all be proud of.

I am also pleased with the outstanding results from our cooperative experiments with international partners including the European community, Japan, and Switzerland. They certainly provide a strong basis for our pursuit of the International Thermonuclear Experimental Reactor project.

One of the challenges the Department had to meet was a Congressional direction in Fiscal Year 1988 to submit a five-year plan showing how the Department will complete the experiments at the Tokamak Fusion Test Reactor and construct the Compact Ignition Tokamak facility within current budgetary resources.

I just received a copy of this plan a few weeks ago, and although we haven't had a chance to review it in detail, I am aware that under budgetary constraints some experimental programs will have to be conducted at a much slower pace than originally anticipated. I am not sure how this would affect our leadership position in fusion research. Furthermore, the three new facilities proposed by the Department all carry high price tags, with only a small percentage of their funding appropriated for this year. How we can secure out-year funding is another issue we will have to wrestle with.

I look forward to hearing some ideas as to how we can resolve these issues from our witnesses today.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Morrison, and I know that you want to join me in welcoming Mike McCormack, the former chairperson of this Committee with us this morning and the author of our fusion bill. It is always good to see you here, Mike.

Mr. MORRISON. I think I've commented before, he was both the father and mother of the fusion energy program and he has resented that sort of a bisexual connotation.

Mrs. LLOYD. I'm not sure I blame him, and I do request unanimous consent that statements from Dr. John Sheffield from the Oak Ridge National Laboratory, Dr. Rulon Linford, Los Alamos National Laboratory, and Dr. Stephen O. Dean from Fusion Power Associates be accepted for insertion into the record. Also my correspondence with Dr. Decker. Dr. Decker, this is your hour. Again, thank you for being here twice in one day.

STATEMENT OF DR. JAMES F. DECKER, ACTING DIRECTOR, OFFICE OF ENERGY RESEARCH, U.S. DEPARTMENT OF ENERGY, WASHINGTON, DC; ACCOMPANIED BY DR. JOHN F. CLARKE, ASSOCIATE DIRECTOR FOR FUSION ENERGY, OFFICE OF ENERGY RESEARCH

Dr. DECKER. Madam Chairman, Members of the Committee, I am pleased to have this opportunity to speak with you this afternoon about the Department's Fiscal Year 1989 budget request for the Magnetic Fusion Energy Program.

I have with me Dr. John Clarke, who is the head of the Office of Fusion Energy, and I have asked him to make some brief comments on the progress of this program. I have submitted written testimony for the record and——

Mrs. LLOYD. Without objection, your entire statement will be part of the record and you may proceed, as well as Dr. Clarke, as you wish in presenting your testimony.

Dr. DECKER. Thank you.

Fusion is one of only three inexhaustible sources of energy to meet Man's long-range energy needs. The other two are fission and solar energy. The goal of the Department's program is to establish the science and technology base for a decision early in the next century to undertake the engineering and development of fusion energy.

In order to meet this goal, the program has been structured to address four key issues: the magnetic confinement systems, properties of burning plasmas, nuclear technology of fusion systems, and materials for fusion systems.

In pursuit of its energy goal, the Magnetic Fusion Program must address a number of frontier scientific issues. Therefore, the program is also an excellent scientific program, as was noted in the 1986 study by the National Academy of Sciences entitled "Physics Through the 1990s."

As an energy program, fusion research has made excellent progress towards this goal this year, and Dr. Clarke, as I said, will describe some of this progress for you in a minute.

To continue our progress into the future, the Department's FY 1989 budget request includes funding for design and construction of the Compact Ignition Tokamak. This device will address the most fundamental remaining scientific issues of fusion research, the physics of a burning plasma. It will be the first experiment in the world to create an actual ignited plasma, a plasma that is sustained at high temperature by fusion reactions.

It's importance to the world program has been endorsed by the Department's advisory committees, both the Magnetic Fusion Advisory Committee and the Energy Research Advisory Board, by the Economic Summit Nations' Fusion Working Group, and just recently by an international review committee composed of experts from Europe, Japan, and the Soviet Union, as well as the United States.

I think an important observation that emerged from this latest review was that the Compact Ignition Tokamak would probably be the only deuterium-tritium burning experiment in the world during the late 1990's, that can carry forward the momentum of fusion research into the next century, when, at that time, we will need an engineering test reactor.

The Compact Ignition Tokamak will use the TFTR facility and a staff at Princeton University. However, the project is organized as a national project. The responsibility for various systems would be assigned to the institutions with established expertise in that particular area.

The Department's 1989 budget request also includes funding of $16 million to continue our participation in an international effort to prepare a conceptual design report and supporting research and development for an International Thermonuclear Experimental Reactor, or, as it is known, ITER.

The ITER program is based on a U.S. proposal that was developed after encouraging discussions between President Reagan and Secretary Gorbachev at the Geneva Summit. The proposal was for the United States, Europe, Japan, and the Soviet Union to participate in a three-year effort to prepare a conceptual design in validating research and development for ITER, an engineering test reactor, under the auspices of the International Atomic Energy Agency. I want to emphasize that there is no commitment to go beyond the conceptual design. The European Community, the Council of Ministers, and the Japanese Cabinet Council have approved their participation in this effort, and Mr. Gorbachev has indicated that during the Washington Summit that the Soviet Union would also participate.

World support for this conceptual design study has been most gratifying. We can already begin to see the benefits that the ITER activity can bring to fulfilling the U.S. strategy of utilizing the maximum amount of international collaboration to achieve fusion energy in a timely manner.

Dr. Clarke will describe the preparations for the activity that have already taken place. In summary, I believe that the Magnetic Fusion Program is working successfully to meet its goal of establishing a science and technology base for fusion. It's two major initiatives are important for meeting this goal, the Compact Ignition Tokamak, as a mid-term domestic program, and the ITER design as a basis for a possible long-range international program.

I appreciate the support that this Committee has given to magnetic fusion over the years and would be happy to answer any questions you might have.

Mrs. LLOYD. Thank you, Dr. Decker.

[The prepared statement of Dr. Decker follows:]

Statement of James F. Decker

Acting Director

Office of Energy Research

Department of Energy

before the

Subcommittee on Energy Research and Development

of the

House Science, Space and Technology Committee

March 24, 1988

Madam Chairman and Members of the Subcommittee:

I am pleased to be here today to discuss the FY 1989 budget request for the Magnetic Fusion Energy program. This Program is an important part of the Department's efforts to assure an adequate supply of energy for the Nation at a reasonable cost. The Program's goal is to establish the scientific and technological base for fusion energy early in the next century. This schedule would allow a timely assessment of the role of fusion energy in the Nation's energy future.

The Magnetic Fusion Energy program is being conducted according to the Magnetic Fusion Program Plan which was developed in 1985. This Plan took into consideration the constraints on Federal spending, the uncertainty of energy supplies, the excellent technical progress in fusion research, the Program's ability to train creative scientists and engineers, the contribution of fusion to basic research in the fields of plasma physics and atomic physics, and the important role of this Program in technology transfer to industry.

The Program Plan elaborates on the strategy for providing the scientific and technological base for fusion. This two-fold strategy is: 1) to maintain a strong domestic R&D program to adequately cover the necessary range of fusion science and technology issues and 2) to use international collaboration to advance the program in a timely way.

The Fusion Program Plan and the Department's strategy for meeting its goal have undergone several reviews recently. The Department's Energy Research Advisory Board (ERAB) conducted its Congressionally mandated triannual review in the last year. ERAB also looked at fusion as part of its review of the National Research Council report "Physics Through the 1990's." In addition, the Magnetic Fusion Advisory Committee held numerous reviews. All of these groups have endorsed the Department's Plan and strategy for meeting its fusion goals.

A strong domestic program, as called for by the Department's strategy, must include work on each of four key technical issues: the properties of burning plasmas, magnetic confinement systems, fusion materials, and fusion nuclear technology. The FY 1989 budget request provides for a U.S. Magnetic Fusion Energy program focused clearly on these key technical issues and interwoven with other world fusion programs through international collaboration. Let me take a moment to comment on these issues.

The first issue concerns the properties of burning plasmas. An understanding of these properties is required to complete the scientific base for fusion. No experimental facility presently exists anywhere in the world to investigate this fundamental issue. The United States is in the best technical position to proceed with such an experiment, utilizing the scientific personnel and infrastructure at Princeton Plasma Physics Laboratory (PPPL). As part of joint international planning to avoid

duplication of costly facilities, the European Community and Japan have encouraged the United States to undertake a Compact Ignition Tokamak (CIT) to provide critical information to support operation of a next step engineering test reactor, now called the International Thermonuclear Experimental Reactor (ITER). The ITER is essential for completing the technical base of fusion. The U.S. has proposed a joint effort, including the Soviet Union, Japan, and the European Community, to develop a single conceptual design for an ITER by the end of 1990. I will discuss the status of this proposal later in my statement.

Achievement of ignition has always been a major objective of magnetic fusion research. Furthermore, CIT is the only proposed experiment in the world-wide program aimed at addressing the most fundamental scientific issue for fusion, that of understanding burning plasma physics. It is also the natural and cost-effective follow-on to the Tokamak Fusion Test Reactor (TFTR) program. We have now reached the point where experimental progress justifies proceeding with an ignition experiment. The Department believes that CIT is critical to the development of fusion energy and to the U.S. Magnetic Fusion Energy program, and it should be noted that CIT is the only deuterium-tritium experiment proposed for the period between the TFTR in the U.S. and a future engineering reactor.

The schedule for the CIT project, for which the architect-engineering design has been initiated this fiscal year, is influenced by both domestic and international program considerations. A timely U.S. CIT would provide

vital information to the world program on how to operate an ITER in the ignited mode and would, thus, save several years of costly exploratory research on the reactor itself. The CIT schedule should allow the facilities and personnel at PPPL, which will complete the TFTR program in 1991, to be productively employed in addressing the important issue of understanding burning plasma physics. The Magnetic Fusion Advisory Committee, the Energy Research Advisory Board, and an international scientific review team have all endorsed the scientific importance of this experiment. The funding proposed in this budget request would allow the U.S. to use the information base available from current experiments both to prepare for its own next step and to support the world fusion program.

The second key technical issue concerns magnetic confinement systems. Although significant progress continues to be made in understanding the confinement process, we do not have enough scientific knowledge at this time to design a fusion reactor that is suitable for commercial application. We must, therefore, continue to investigate several confinement systems at modest levels in order to provide this scientific knowledge. Research on this key issue is being closely coordinated internationally to maintain the broadest scientific coverage at minimum cost.

The third key issue concerns materials for fusion systems. Materials research is vital to a successful experimental Magnetic Fusion Energy program, as well as to realizing the ultimate benefits of fusion.

Achievement of the Program goal will require the development of new materials to reduce the induced radioactivity and, thereby, enhance the economic and environmental potential of fusion, as well as the development of the facilities required for testing these materials. As part of the Program's international strategy, this issue is being pursued through cooperative agreements. The proposed FY 1989 budget supports the core program needed for U.S. participation in this international effort.

The fourth key issue concerns the nuclear technology of fusion systems. Resolution of this issue requires advances in the basic engineering sciences, as well as application of the results of basic fusion materials research. This issue will be completely resolved only when fuel-producing blankets are integrated into other fusion systems and tested in a nuclear environment, such as ITER. A fusion blanket performs several functions, including converting the energy released by a fusion reaction into useful energy, creating part of the fuel for the reactor, and allowing for recovery of this fuel, as well as shielding critical components from radiation. Our FY 1989 budget request supports the basic technology research that underlies the development of integrated blankets for the long-term and for the ITER conceptual design and validating R&D process in the near-term.

International collaboration has become a major resource for the development of fusion energy. The fusion program has a long history of scientific exchange and cooperation. The Economic Summit process has provided a

mechanism for developing an integrated fusion program for the Western allies. Through a series of important international collaborative arrangements, the program has made a great deal of progress toward this goal in the past four years. As noted earlier, the U.S. has proposed a joint effort including the USSR, Japan and the European Community to develop a single conceptual design for the ITER by the end of 1990. After formal discussions among the four parties, the International Atomic Energy Agency issued an invitation to the parties to participate. The U.S., Japan, the European Community, and the USSR all have accepted the invitation. We plan to begin a joint design effort on May 2, 1988. This budget provides for continued U.S. participation in the conceptual design and validating R&D for an international ITER during FY 1989.

The FY 1988 Appropriation Act directed the Department of Energy to submit a five-year budget plan showing how the Compact Ignition Tokamak could be built while completing the Tokamak Fusion Test Reactor program within current budgetary resources. A summary of the plan is included in the FY 1989 budget (Table 1). This plan has been developed recognizing the need for both a balanced base program and preparation for a future program. It focuses the U.S. Magnetic Fusion Energy program strongly on ignition physics issues based on TFTR and CIT. The plan maintains a limited number of recent initiatives aimed at improving toroidal confinement and continues awareness programs in long-range technology. International commitments, including participation in ITER, will all be met under this plan.

Table 1
MAGNETIC FUSION ENERGY
OUTYEAR BUDGETS/(ESCALATED DOLLARS)
(dollars in millions)

	FY 1988	FY 1989	FY 1990	FY 1991	FY 1992	FY 1993
Compact Ignition Tokamak Total	15.5	27.0	46.0	64.0	95.0	101.0
Tokamak Fusion Test Reactor	72.1	74.3	83.0	79.0	45.0	42.0
Total Ignition	(87.6)	(101.3)	(129.0)	(143.0)	(140.0)	(143.0)
Princeton Beta Exp.	11.1	11.4	-	-	12.0	13.0
Alcator C-Mod.	10.1	15.5	16.0	16.0	17.0	17.0
Adv. Toroidal Fac.	18.1	18.0	19.0	19.0	20.0	21.0
DIII-D	29.7	32.5	33.0	35.0	46.0	48.0
Microwave Tokamak Exp.	9.3	10.5	10.0	11.0	-	-
International/Other	7.2	6.6	6.0	7.0	8.0	8.0
Mirrors	3.6	3.0	3.0	-	-	-
Conf. Sytems Total	176.7	198.8	216.0	231.0	243.0	250.0
Base Dev. & Tech.	42.4	42.3	41.0	40.0	41.0	44.0
International Thermonuclear Exp. Reactor**	16.0	16.0	16.0	17.0	18.0	19.0
Dev. & Tech. Total*	58.4	58.3	57.0	57.0	59.0	63.0
Applied Plasma Physics*	78.0	80.6	79.0	80.0	82.0	87.0
General Plant Projects/ General Purpose Equip./ Program Direction	21.9	22.3	23.0	24.0	25.0	26.0
Office of Fusion Energy Total	335.0	360.0	375.0	392.0	409.0	426.0

* Basic R&D in support of the CIT program will be continued, as appropriate.
**Conceptual design report for ITER to be completed in 1990; outyear projections include level of effort, no construction.

In summary, this budget provides for a U.S. fusion program focused clearly on the key technical issues and interwoven tightly with other world fusion programs through international collaboration.

FY 1989 MAGNETIC FUSION ENERGY BUDGET REQUEST

The FY 1989 budget request for Magnetic Fusion Energy is $360.0 million. The request includes $317.5 million for Operating Expenses, $21.6 million for Capital Equipment and $20.9 million for Construction (Table 2). This request will support experimental investigations that are essential to continued research progress and will maintain a balance between our domestic science and technology program and foreign programs with the goal of allowing international collaboration to supplement our domestic research activities.

The request of $360.0 million will allow us to continue work on the four key technical issues discussed earlier in my statement. It will support construction of the Compact Ignition Tokamak device and efforts to develop a common international conceptual design for ITER. To maintain progress at this level of funding, the Program continues to be focused primarily on the investigation of toroidal confinement systems. In addition, we continue to focus the Program on the highest priority tasks, improve the cost effectiveness of facility operations and increase the use of international collaboration in all areas.

Table 2
Magnetic Fusion Energy
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses	$315.4	$298.4	$317.5
Capital Equipment	18.0	19.7	21.6
Construction	8.2	16.9	20.9
Total	$341.6	$335.0	$360.0

The Operating Expenses request of $317.5 million reflects about a 6 percent increase. This increase provides for constant dollar funding for most elements of the program and for increases to meet project funding requirements associated with R&D support and construction of the CIT, preparations for deuterium-tritium operations in TFTR, fabrication of the Alcator-C Mod device, and of the Confinement Physics Research Facility. Work in the technology development area will focus on only the most critical feasibility issues, while providing support for CIT and the Presidential initiative to design and conduct validating R&D for ITER.

Let me now summarize the Operating Expenses request for each of the interrelated functional areas within the Magnetic Fusion Energy program: Confinement Systems, Applied Plasma Physics, Development and Technology, Planning and Projects, and Program Direction (Table 3).

The Confinement Systems subprogram, with an FY 1989 Operating Expenses request of $175.6 million, supports large-scale physics experiments to address key physics issues associated with heating and containing high-temperature plasma. Confinement Systems also develops an information base on reactor-relevant plasma conditions.

In FY 1989, continued studies related to the confinement of high-temperature plasmas will be conducted at the TFTR at Princeton Plasma Physics Laboratory, with preparation for the deuterium-tritium breakeven experiment and studies of plasma confinement at increasing power levels.

Table 3
Magnetic Fusion Energy
Budget Authority
($ in millions)

	FY 1987	FY 1988	FY 1989 Request
Operating Expenses			
Confinement Systems	$184.8	$158.6	$175.6
Applied Plasma Physics	75.2	74.8	78.2
Development and Technology	50.5	55.9	54.1
Planning and Projects	0.9	4.5	5.0
Program Direction	4.0	4.6	4.6
Subtotal	$315.4	$298.4	$317.5
Capital Equipment	18.0	19.7	21.6
Construction			
Compact Ignition Tokamak	0	8.0	12.0
General Plant Projects	8.2	8.9	8.9
Subtotal	$ 8.2	$ 16.9	$ 20.9
Total	$341.6	$335.0	$360.0

TFTR has already achieved central plasma ion temperatures of over 300,000,000°C, a value which is more than adequate for a commercial thermonuclear reactor; and also has achieved world record values of the fusion confinement parameter which are also adequate for achieving ignition. Breakeven with deuterium-tritium fuel is expected in 1991. The FY 1989 Operating Expenses request also continues the design and R&D effort aimed at building the CIT. Aspects of the CIT design, experimental objectives, and physics studies are specifically directed, wherever possible, to be relevant to ITER.

At the DIII-D device at General Atomics, scientists will continue to explore the ratio of plasma energy to magnetic energy, a ratio characterized as "beta." Higher magnetic fields require larger, more costly magnets. Therefore, experiments are carried out with alternate plasma shapes and operating modes predicted to increase beta. In 1987, a world record beta of 6.2 percent, compared to the previous record of 5 percent, was achieved and sustained without disruptions for several hundred milliseconds. A vigorous experimental program to assess the properties of high-temperature, high-pressure, non-circular plasmas in support of CIT design work is continuing at DIII-D in collaboration with the Japanese.

In addition to the facilities already mentioned, fabrication of the Alcator-C Modification at the Massachusetts Institute of Technology (MIT) also will continue. This facility will confirm various aspects of the CIT design, physics assumptions, and projected operating modes. The Microwave

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Tokamak Experiment, being fabricated at Lawrence Livermore National Laboratory (LLNL) using the old Alcator-C device from MIT, will be completed in FY 1988 with initial operation expected in FY 1989. This device will provide the unique capability to test the application of pulsed high power microwaves as a more efficient heating technique in tokamak devices. The FY 1989 request also provides for continued operation of the Advanced Toroidal Facility (ATF) stellarator at Oak Ridge National Laboratory (ORNL) and the Princeton Beta Experiment at PPPL. The Major Device Fabrication for the ATF was completed in December 1987. Plasma operation was initiated in January 1988 with 200 KW of electron cyclotron heating, and neutral beams were injected in February. Research at ATF will contribute to the advancement of toroidal physics and to the international collaborative effort on stellarators.

International collaboration will be relied on to carry out research on a number of related plasma issues in foreign facilities including TEXTOR and ASDEX in West Germany, Tore Supra in France, the Joint European Torus (JET) in Europe and JT-60 in Japan. In just one example of the ongoing collaboration at these facilities, ORNL and PPPL have fabricated and installed a 3-barrel, high speed pellet injector on JET as a part of the U.S.-European Community cooperation. Last December, the first in a series of pellet injection experiments resulted in the highest density in a JET plasma with ion cyclotron heating. These experiments, which are essential to achieving the required high fusion confinement parameter, are continuing.

The Applied Plasma Physics subprogram, with an FY 1989 request of $78.2 million, develops plasma physics theories and models to predict and explain the experimental and theoretical investigations of basic physics problems related to confinement concepts. This subprogram also conducts research on magnetic confinement configurations with potentially improved reactor features, provides computational support to the program, and develops diagnostic instruments for use by the Confinement Systems subprogram.

Significant progress has been made in understanding physical processes that reduce confinement of particles within tokamaks. Several advanced diagnostic techniques were applied by a collaboration of university groups at the TEXT tokamak to obtain the new measurements. Through theoretical efforts, these insights will impact our understanding of the critical, related issue of energy confinement in tokamaks.

Exploratory experiments on a small toroidal device recently found highly efficient production of the plasma current, which is required to form the tokamak configuration, through application of an electron "beam" at the plasma edge. Improved current drive efficiency is a critical reactor feature for tokamaks. These initial experiments will be advanced to a moderate sized tokamak during FY 1989.

The spheromak, a compact toroidal plasma with no hardware on the inside of the torus, is one of the alternate confinement concepts being investigated under this subprogram. The S-1 experiment at Princeton has completed a

4-year research program that demonstrated plasma compression heating, one of several methods of heating the plasma, and confirmed the pioneering theory of "energy minimum" confinement. This theory determines generally how the magnetic fields and stored energy evolve in all toroidal plasmas.

Other alternative confinement concepts that will continue to be investigated in FY 1989 are the Reversed Field Pinch (RFP) and Field Reversed Configuration (FRC). Both devices make efficient use of magnetic fields. In addition, RFP devices require no auxiliary heating, while FRCs have simple magnetic fields and offer natural systems for impurity control. The FY 1989 request for the Applied Plasma Physics subprogram reflects a slight increase primarily to continue fabrication of the Confinement Physics Research Facility (CPRF) at Los Alamos National Laboratory. Support for the theory, experimental research and computer network elements of this subprogram will continue at a flat level with some decreases in scope of work to accommodate the increased cost of operations.

The Development and Technology subprogram, for which $54.1 million is requested in FY 1989, develops components and systems for use in near- and mid-term fusion experiments, as well as long-term generic materials and fusion nuclear technology. This subprogram also performs conceptual design studies for future fusion devices. These design studies identify component and system development needs that must be met to prepare for the eventual application of fusion.

In FY 1987, the International Energy Agency Large Coil Task testing program at ORNL was completed to the satisfaction of all participants, the U.S., Japan, the European Community, and Switzerland. Based on the results of this program, we are now confident that we can design and build the required large superconducting magnets for fusion.

Organization of the U.S. ITER activities has been completed, including Technical Study Tasks and R&D to validate design concepts. In FY 1989, design and R&D work will continue in support of ITER. These efforts will include international collaboration on development of high field, steady state magnets; research on improved techniques for heating, current drive and plasma control; development of higher performance pellet injectors; research on tritium recycling technology and safety and environmental issues; and materials development in support of the ITER design.

The Planning and Projects subprogram primarily provides support for construction of major new facilities and fabrication of auxiliary components for these facilities. The FY 1989 Operating Expenses request for $5.0 million is for the support of special program needs and for compliance with legislative requirements associated with the Small Business Innovation Research (SBIR) program.

The Operating Expenses request of $4.6 million for Program Direction provides for personnel and other costs associated with continuation of 62 full-time equivalent staff.

The FY 1989 Capital Equipment request of $21.6 million will provide for essential hardware necessary to support the Program, such as power supplies, diagnostic and computer equipment and components for experimental facilities. The increase in the Capital Equipment request is primarily in the Toroidal Confinement Systems area and the Development and Technology area.

The FY 1989 Construction request of $20.9 million provides $12.0 million for the Compact Ignition Tokamak that I discussed earlier. A national design team has been working for several years to develop a low-cost concept that would provide a compact, high field, copper coil tokamak facility to generate critical burning plasma data to allow for successful operation of an integrated ITER. The project, with a TEC of $426 million, began in FY 1988 and is scheduled to be completed in FY 1996. The funds requested for FY 1989 will provide for design, long-lead-time procurement, and initial construction of the CIT device components and systems. The remainder of the Construction request, $8.9 million, is for General Plant Projects. These projects at LLNL, PPPL and ORNL provide for the continuing minor alterations and modifications necessary to protect the government's investment in its facilities.

That concludes my statement. I will be happy to answer questions.

Mrs. LLOYD. Dr. Clarke.

Dr. CLARKE. Madam Chairman, as you reminded the Committee, this does mark the tenth year I have had the opportunity of testifying before your Committee, and I would like to join Dr. Decker in expressing my appreciation for the support you have given us over the years. One concrete way that I have thought of expressing that appreciation is by trying to illustrate the progress the program has made over that decade.

If we can cast our minds back for a moment to 1978, if you'll recall, we had just begun the construction of the TFTR facility. The goal of that machine that we put forward was to investigate energy break-even conditions and to produce between one and 10 megajoules of fusion energy.

Now looking back over a decade, it is easy to forget how bold a step that was at the time. In order to achieve the goals that we set for that machine, it required us to increase the plasma temperature from what had been achieved in the largest similar machine by a factor of 5. We had to increase the energy confinement by a factor of 20, and we had to increase the plasma pressure by a factor of 5 and the energy gain by a factor of 50. These are all very, very large numbers.

This year I'm very happy to say that TFTR has achieved plasma conditions that reached the original goal by the production of three megajoules of fusion energy. We have also produced and in many ways exceeded scientific results that we only hoped for 10 years ago—300 million degree temperatures, which were two and a half times the original goal of TFTR; confinement exceeding the Lawson criterion in approaching that required for ignition, ten times the original TFTR goal; and perhaps more important, we have learned much that we didn't even suspect or expect to find 10 years ago.

We have been able to incorporate this new information into the experiments through modifications over the years and we have been able to achieve 25 percent of energy break-even in the machine. Based upon a recent review by the Magnetic Fusion Advisory Committee, we hope that we will be able to extend these results over the next few years to achieve between 50 and 100 percent of energy break-even in a deuterium-tritium plasma. We will get the first indications in a deuterium plasma this coming fiscal year.

Now the TFTR performance, I think, is the most dramatic example of progress, because that represents the production of a real fusion grade plasma. However, as we have proposed in our budgets over the last few years, there are equally important questions that have to be investigated in other parts of the program, and we have made a lot of progress in these areas as well.

For example, I believe Mr. Morrison mentioned the achievement of the 6.2 percent magnetic efficiency value, or beta, in the General Atomics experiment DIII-D. This magnetic efficiency is the ratio of plasma pressure to magnetic field pressure, and as you know, it is a measure of how well a Tokamak could perform as a reactor, and the 6 percent level is past the minimum level at which we could hope to actually produce a practical fusion reactor. So that is a very significant achievement indeed.

Ten years ago, the achieved magnetic efficiency was less than one-half of one percent, and in fact our theorists were predicting ultimate limiting values for the Tokamak of only a few percent. We have since learned how to surpass these pessimistic expectations experimentally through a lot of hard work by a lot of dedicated experimentalists. Our scientific understanding has improved as a result. We now have theoretical calculations predicting that we can extend these results to the 10 percent range, and, in fact, theorists have come up with new ideas that indicate that we might be able to surpass even that level of beta in the so-called second stability regime. We have a number of experiments in the program that are addressing this particular aspect of fusion.

Now, in addition to the plasma physics, we have also made a lot of remarkable progress in the technology of magnetic fusion. One example of this is one that you are very familiar with, the Large Coil Project at Oak Ridge National Laboratory. This project has yielded a wealth of data on our ability to manufacture and utilize superconducting magnets in Tokamak configurations. I think it is very significant that this was an international project. It was conducted under an IEA agreement and the coils were contributed by Euratom, Switzerland, Japan, and the United States.

Each of the magnets, no matter where they came from, exceeded their design specifications, and I would like to indicate that I believe the U.S. has benefitted greatly from this collaboration. We have learned, we have benefitted from the hardware contribution certainly, but we have also learned about the engineering design and the technological base for superconducting magnets from our foreign colleagues. I think that statement would equally well be made by any of the participants. All parties believe that this was a very successful and mutually beneficial example of collaboration.

Now this project is one striking example of the progress that we have made over the last decade in international collaboration. Working alone, we simply could not have achieved the breadth of understanding and the technology base in superconducting magnets that we have from this cooperation, nor could we have achieved that level of understanding in a timely manner without the collaboration.

Another example that illustrates both the progress in fusion technology and international collaboration is the Tritium Systems Test Assembly experiment at Los Alamos. Ten years ago we had just decided to start that facility. Last year tests were completed using 100 grams of tritium in that facility. That is five times the amount of tritium required for CIT, just to put it in perspective. The United States and Japan had signed a $10 million, five-year agreement to jointly share the operating costs of that facility. That agreement should enable us to continue progress in that vital area of technology for the next five years.

Looking to the future, we have started the final experimental campaign to study actual deuterium-tritium operation on TFTR, and I know to some Members of this Committee it seems like a long time—that we have waited a long time for that, but the progress has started. Now this is a very important and complicated task, and it will require several years of very careful preparation to successfully complete.

This year we will complete the tritium handling system and the remote maintenance manipulator, and, in the Fall of this year, we will perform initial tests of the tritium system with one one-hundredth of gram of tritium fuel. By July, 1990, we hope to have full hardware capability on the TFTR, and we will finish the systems tests using a tenth of a gram of tritium. Final experiments are scheduled to begin in January of 1991.

If all goes well, these experiments will introduce fusion research to the burning plasma physics era, the final frontier of fusion science which we hope to complete the exploration of in the CIT in the late 1990s.

In addition to these specific examples I have mentioned, I would like to close with a few remarks illustrating the general progress that we have made in achieving international collaboration. As noted in the OTA report, "Starpower", international collaboration is vital to timely progress in the U.S. fusion program. Today we have more than 100 specific cooperative activities between the United States and Japan, including nearly a dozen collaborative projects. There are six substantial International Energy Agency programs. There is a thematically—oriented U.S.-U.S.S.R. bilateral program and a growing set of U.S.-European community activities. Fusion is now regularly referenced at the ministerial meetings of the IEA as a paradigm of technical cooperation.

Recognition of the role played by cooperative ventures is also improving. This has led to the encouraging statements at all of the U.S.-U.S.S.R. summits in recent years, and this has resulted in the Presidential initiative on ITER that Dr. Decker referenced.

I am happy to report that preparations for this activity are going extremely well. The activity will begin officially on April 21 of this year. I have just recently been informed that the Soviet Union has officially accepted participation in the activity.

By May 2, ten people from each of the participants will begin working at Garching, West Germany, for a 6-month initial design definition activity. The preparatory activities have largely been completed due to the enthusiastic response of the world scientific community. The European Community has built and equipped a first-class engineering design facility to serve as a site for these joint design activities, and the ITER managing directors have identified a very excellent and high caliber team of international engineers and scientists who have agreed to spend the next six months in West Germany working together on this initial phase.

Each of the participants has also organized a very high caliber home team which will support the ITER participants in West Germany. The core group, supported by the rest of the ITER teams at the home sites, will define the ITER experiments by this Fall and then direct the detailed design of the experiment over the next two years.

We have established a world-wide electronic mail system linking all of the participants, and we have tested it, and a data network for the engineering activity is nearly in place. I think the ITER activity promises to advance both the engineering and international cooperation aspects of fusion. If successful, it would be a major step forward towards that unprecedented level of international collabo-

ration that the OTA "Starpower" report saw as a necessary element in achieving our domestic goals in fusion.

In conclusion, as a result of the past support of this Committee and other Committees of the Congress, we have completed 10 years of what I believe is dramatic technical progress in magnetic fusion. It has brought us to the study of break-even plasmas. With your continued support, we are about to embark on a decade-long effort to investigate the physics of ignited plasmas, the next scientific step in fusion. And finally, we are well advanced in building a foundation of international collaboration that we see as necessary to establish the science and technology base for practical fusion power early in the next century.

Thank you very much.

DISCUSSION

Mrs. LLOYD. Thank you very much for your excellent statement as well, John. It occurred to me while you were reviewing the progress of the past ten years, I would like to put a statement in the Congressional Record of the progress that has been made, certainly it is a source of pride, not only for you and the fusion community, but for certainly this Committee as well.

You mentioned the need to go forward with more materials research, that is one of the goals still ahead of us. At the same time, the High Flux Isotope Reactor at Oak Ridge, as you know, is down. If Oak Ridge does ask permission to restart this summer, what do you anticipate your reaction to be, Dr. Decker?

Dr. DECKER. We are working with the Oak Ridge Operations Office, with the contractor, to try to get that reactor started up in the June/July timeframe. We are very hopeful we can restart at that time. As you know, we have had a series of problems with our reactor that need to be straightened out. The Department certainly is not going to restart that reactor until it feels it is safe to do so.

Mrs. LLOYD. Is there anything further we can do to counteract this problem?

Dr. DECKER. I think we have a good plan in place now with milestones to head towards a restart. Like I say, it looks like that restart time will be around July.

Mrs. LLOYD. Very good. As I mentioned in my opening statement, a review of the CIT in January by an external group indicated some concerns about the design of the CIT to achieve a burning plasma. I would like you to comment on the recent changes in the CIT design based on that review and its implications.

Dr. CLARKE. I think that, since Dr. Furth is testifying following us, that he will probably give you some remarks on the status of the design and principle.

Mrs. LLOYD. I wanted your perspective as well.

Dr. CLARKE. Our perspective is that we found that to be a very useful review. It really was the first time in which an international body of fusion experts publicly stated their support for the work that we are trying to accomplish on the CIT. Their basic concern was that since the CIT will be the central world facility for the study of burning fusion plasmas in the late 1990's, they wanted to

be assured, to buy insurance, as it were, that the device would actually be capable of investigating those conditions.

Of the people on the review committee—from the people on the review committee, we got a number of suggestions, things they thought would add to the capability of the device, and they urged us to seriously consider it. At the end of their comments, they did remind us that, of course, the people at Princeton are responsible for the success of the device and they would be the best judges of which of these suggestions actually to incorporate.

We are trying to take into account everything that the Committee said and the approach that we have adopted is to try to maximize the ultimate capability of this device while keeping the near term commitment for the cost of the device to a minimum.

Mrs. LLOYD. That is what I was going to ask about, the cost impact, if you would comment on that.

Dr. CLARKE. We won't be sure of the exact cost impact of these changes until we have our cost and schedule design review which we anticipate in the mid-summer timeframe. It looks as if we will be able to incorporate many of the changes suggested by this Committee and postpone paying for the extra capability until such time as we see from our experiments whether or not we actually have to go ahead and implement this.

What I am talking about is the idea of designing in the capability for higher magnetic fields and postponing the purchase of the power supplies and things of that nature, which could be added at a later time if we find it to be necessary.

Mrs. LLOYD. You would reserve judgment in increasing capabilities?

Dr. CLARKE. I think it is a question of balancing costs and risks. If you ask 100 fusion experts, you will probably get 100 fusion answers, from the pessimistic to the optimistic. What we are trying to do is balance off the U.S. Treasury's obligation with the capability of the device. From what I've seen so far, I think Princeton is doing an admirable job in achieving that.

Mrs. LLOYD. I am going to ask for your opinion on this. Do you personally feel from your professional judgment that the CIT can now achieve a fully ignited self-sustained burn?

Dr. CLARKE. You are asking me to calibrate myself in this range of opinions. I'm generally a middle of the roader. I think from what I have seen of the design so far, I think we will be able to investigate the physics of burning plasmas. Let me elaborate a little bit.

As you increase the energy gain of these devices, you produce more and more alpha particles. The scientific question that we are trying to investigate is the effect of those alpha particles upon the dynamics of the plasmas. That is the real scientific question. In that sense, it doesn't matter whether we get to the state of turning off the external power and letting the plasma just burn by itself or whether we can operate with an energy gain of, let's say, ten.

My confidence in the CIT to achieve its mission, scientific mission, is really based on my assessment that in the worse possible case, the worse possible case that we now envision, I believe the present device will be able to produce a very high energy gain plasma and thus satisfy its scientific mission.

Mrs. LLOYD. Very good. Both of you spoke of international collaboration as one of the highlights of the program or one of the great resources. As you know, we have been somewhat skeptical on this Committee about some of the international progress, how it really relates to the program. I would like for you to really give us a little more detail on the ITER activities to date, how it is really benefitting our program. I know you spoke of materials and engineering design. What can we really realize in terms of a construction project?

Dr. DECKER. I think it is hard to know at this time what will be realized in terms of a construction project that might come out of this. At the time we complete the conceptual design, which is a three-year effort, it would allow all the participants, I think, to examine whether or not they wish to proceed with construction of the device.

It is conceivable that we might join with one other country or any two of the participants or maybe all of them. I think that will depend on an awful lot of the various conditions at the time, international relations with various countries being one of them.

Mrs. LLOYD. We have a new Member, Mr. Hochbrueckner, and we are happy to have him with us. He is a new Member on our Committee and we look forward to him playing a role in the activities of this Committee as well.

If international collaboration is such a great idea, why can't we use the result of the proposed deuterium-tritium experience on JET and free up TFTR to do more physics? What objectives would we not meet if we go this route?

Dr. CLARKE. We have been struggling with that issue for a number of years. Given the cost constraints on the program, it is a very relevant question. We finally posed almost exactly that question to our Magnetic Fusion Advisory Committee. We asked them to look at the international capability of learning the physics, doing the physics of TFTR and really whether or not we should introduce tritium into that device. Their advice to us was in two parts.

The first part was that, scientifically, TFTR will just begin to introduce the subject of alpha particle physics. It certainly will not go very far in that direction. It will give us the first taste. They felt that the results of TFTR in that area were scientifically significant but it is quite true that ultimately JET, which is a more capable machine, could probably produce those same results.

The second part of their advice was to me the most telling, and that was that considering the experience to be gained from actually operating a deuterium-tritium plasma, considering the fact that the next major goal in the U.S. program is the operation of the CIT device, which would have much more power, much more tritium and be a much more complex D–T device to operate, that it would be really foolish for us to try to proceed without taking advantage of the experience on the TFTR. That to my mind is the most telling argument that supports going ahead with that at this time.

Mrs. LLOYD. Do you feel that is more advantageous, to do more physics?

Dr. CLARKE. I certainly do. I mentioned we had started on the campaign, experimental campaign to exploit D–T physics on TFTR.

This is 1988. We are not going to introduce the real tritium into the machine until January, 1991. All of this intermediate period is going to be devoted to physics research on TFTR, utilizing all of the heating capability and the diagnostic capability that we so painfully built up over the last decade, and I think that we have a realistic expectation of wringing out the physics from that device by the time we introduce tritium.

Mrs. LLOYD. My final question involves money. The budget plan that you proposed to accomplish your goals in CIT and TFTR activities within present resources assumes the level of $360 million. If this budget is cut 10 percent, how would you modify it? I will ask one of you that one and the other one I will ask if the budget were increased 10 percent. You can take your choice.

Dr. DECKER. I think if the program were to be reduced 10 percent, we would certainly want to try to maintain, as best we can, the balance that we do have in the program today. I think that would likely mean some slowing of CIT and some other areas. I think a basic guideline is we would like to maintain the kind of balance that we have in today's program.

Mrs. LLOYD. Just a basic overall cutback?

Dr. DECKER. Yes.

Mrs. LLOYD. What are you going to do with 10 percent more, John?

Dr. CLARKE. I think Dr. Decker's answer is the answer to both questions. As you know, with all of the reviews of the fusion program balance, and content, and technical directions over the past few years, reviews by OTA, reviews by ERAB, reviews by the Magnetic Fusion Advisory Committee, we have not gotten any advice to change the balance of the program.

We recognize very clearly what has to be done to achieve the goal of fusion, and our strategy is to utilize the money that Congress provides in the most effective manner to pursue that program. If we get less, we would intend to pursue the program at a slower pace. If we get more, we can go faster.

Mrs. LLOYD. Your strategy is the same?

Dr. CLARKE. The strategy would not be changed at the 10 percent level.

Mrs. LLOYD. Thank you very much. Mr. Morrison?

Mr. MORRISON. Gentlemen, I read a week or so ago something that I thought had been totally classified about—you are looking at each other. [Laughter.]

I read about something the Committee was aware of and that was some fusion type experimentation that had been done as part of the underground tests at Nevada. I don't know if you can even talk about it, except I have read it in the local papers.

I am wondering if there has been any correlation between that work and your efforts with the various machines that we have been developing over the last 10 years.

Dr. DECKER. There is not much in the way of correlation between what we have been doing in the magnetic fusion program in our devices and the results that were talked about in the paper in the sense that the results talked about there are in inertial confinement, a rather different approach, as you are aware.

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Mr. MORRISON. Was there anything learned through that experimentation that is helpful?

Dr. DECKER. That would impact magnetic fusion?

Mr. MORRISON. Right.

Dr. DECKER. No; I don't believe so.

Mr. MORRISON. Other than the fact that supposedly they achieved a level of reaction that might potentially represent a breakthrough? A break even point?

Dr. CLARKE. Mr. Morrison, there is no direct effect or connection between that work and the magnetic fusion program. On a working scientific level, of course, we have benefitted from the inertial confinement program. The experiment that we are conducting at Livermore on the MTX device involves injection of high-power microwaves into a fusion grade plasma. The non-linear interactions that we expect to encounter in that experiment have all been analyzed using the expertise developed by the inertial program.

This is just an example of the kind of scientific connection, but as far as advancing the program, there really isn't any. It is just a purely scientific understanding connection.

Mr. MORRISON. Have the Soviets shown any interest in the work that was done underground?

Dr. DECKER. Not that I know of.

Mr. MORRISON. I wanted to ask about the engineering side of the issue. I recall when this work was initially undertaken that of course there was a very clear division between the physics aspects and the engineering aspects with one of our hopes out West was a Fusion Materials Irradiation Test Facility. I know you have concentrated in recent years on the physics side of the fusion research. Where are we when we finally create this marvelous reaction and have it ongoing? How are we going to hold it?

Dr. CLARKE. We have not forgotten the materials issue. As you remember, we had this unfortunate experience in that we were not able to continue the work on the Fusion Materials Irradiation Test Facility because of budget limitations. We tried to achieve international collaboration to help us support that facility and were unable to do so.

Our present strategy is to keep working that international angle. The world fusion programs agree that we need—it is absolutely essential for us to have a materials irradiation test facility. We are making some progress in that. We have a working group that we recently organized under the International—or we are in the process of organizing—under the International Energy Agency, to try to build a world community that can generate sufficient support internationally to enable us to go forward with such a facility. The plans that we have laid out indicate that, sometime in the late 1990's, we need to start construction of such a device.

Mr. MORRISON. What is your timeline as we move on out closer to demonstration, potentially getting on line with fusion? Some of the initial concepts, I think, probably shortly after the turn of the century. Are we at 2025? Where are we now?

Dr. DECKER. I think that we've got to go through at least two more steps before we're to the point of establishing both the science and the engineering required for fusion, where we could make a decision on commercialization.

We need to move forward with the Compact Ignition Tokamak, to firmly establish the remaining physics questions. And, secondly, we need to construct an engineering test reactor along the lines of the ITER device.

I think the earliest that one could conceive of completing construction of an engineering test reactor is sometime after the turn of the century, maybe 2005, something like that.

Mr. MORRISON. And if it works, commercialization—

Dr. DECKER. Then commercialization is probably 25 year, 20 year, and up, time period. Introducing a new technology into the energy economy just takes time.

Mr. MORRISON. Well, that's understandable, and I think your ten-year report, particularly Dr. Clarke's, since you've been with us all the way through, is an exciting one.

We are particularly pleased with the international cooperation. I think it's a model. And we'll trust that, ten years from now, you'll still be here. I'm not sure that the Chairman and I will still be here, but we'll come back as Congressman McCormack has to listen to your report with pride.

Thank you.

Mrs. LLOYD. Thank you very much. We thank you witnesses for being with us. Your testimony was excellent, and your company is equally inspiring.

Dr. DECKER. Thank you.

Mrs. LLOYD. Next is Dr. Harold Furth, who is director of the Princeton Plasma Physics Lab, also no stranger to this Subcommittee. And Dr. Furth is going to tell us about progress on CIT, and how our TFTR research relates to CIT.

We welcome you, Dr. Furth. Your entire written statement, as always, will be made a part of the record. You may summarize, or proceed as you wish.

And identify your associates for us, please.

STATEMENT OF DR. HAROLD P. FURTH, PRINCETON PLASMA PHYSICS LABORATORY, PRINCETON, NJ; ACCOMPANIED BY E. C. BROLIN, DEPUTY DIRECTOR FOR TECHNICAL OPERATIONS, AND DR. DALE MEADE, HEAD, TFTR EXPERIMENT

Dr. FURTH. Madam Chairman, it's a great pleasure to be here. I would like to introduce some gentlemen who are sitting beside me.

The Plasma Physics Laboratory, as of recently, has a new Deputy Director for Technical Operations and that is Mr. E.C. Brolin. He used to work with Admiral Rickover at the dawn of fission energy, and he's arrived just in time for the dawn of fusion energy. His expertise in nuclear matters will be of great aid to us.

And on my right is Dr. Dale Meade, who is the head of the TFTR experiment.

Mrs. LLOYD. We welcome both of you gentlemen as well.

Dr. FURTH. In my written testimony I go into detail on certain matters of a technical nature. In my oral statement, in order to keep it brief, I would like to concentrate on just the main issues and try to make as straightforward a reply as I can to the concerns that I heard you raise.

And, if my answer is incomplete, then perhaps in the questions we can pursue it some more.

First of all, I would like to comment on the question, is fusion an energy program? And that takes me back to my first experience with fusion during the 1950's, when fusion research wanted to be an energy program. That meant to them making some fusion energy, which was finally accomplished at the end of the 1950's in the making of one million fusion reactions per shot.

To put that in an intelligible framework as energy, that is the work done by one horse in one nanosecond, one nanosecond being one billionth of a second. So, that is not a great deal of energy.

Further on that point, I should say that, while this work was going on, enormous progress was made in basic science. A whole new field of modern physics was developed. The basic inventions and the basic concepts in confinement theory were produced. So, this was a bright page in modern science, but not exactly an energy program.

Turning to more recent times, last year the Tokamak Fusion Test Reactor, TFTR, produced several times 10 to the 16th reactions per shot in deuterium. And you will be pleased to hear that is one horse working for 20 seconds, which, after all, begins to be something.

If we now went from deuterium to tritium, and had a D–T plasma, we would have one horse working for one hour. And, under the break-even conditions that we plan, there will be 15 horses working for one hour. That will be the energy that will be released.

So, I think, that at least is the beginning of an energy program. Looking ahead to the Compact Ignition Tokamak, each pulse will amount to 500 horses working for an hour. And, I think, even more impressive, if you look at the ITER that you've heard about, that is 1 million horses working much of the time. So, once we reach that point, I think you will agree that is a real energy program.

Now, let me get away from horses and get on to fusion reactions. In fusion we have learned a great deal during the last four decades. We can now describe a reactor in terms of theory and computation in quite considerable detail.

And so, it is logical to ask why do we need the CIT? Why do we need the TFTR? Why do we need the base program? Why don't we just go ahead and build the real reactor, beginning with the ITER?

So, let me take a moment to address that. The principal answer is that our theoretical standing is not complete. And, so, there are bound to be surprises. There always are. Some of these surprises are bad. And that is unfortunate because then things we had counted on don't happen.

Other surprises are good. And that means there are new opportunities that we can only take advantage of if we know about them.

And both of those surprises point to the effectiveness of having a step-wise program that goes from one thing to the next, checking our theories, checking our assumptions, and learning as we go.

Now, that message is particularly relevant in deuterium-tritium-burning plasmas, where, on the first successful shot, the machine becomes radioactive. That doesn't mean that there's a threatening amount of radioactivity. It's actually very small. But it is enough to

keep the experimenters and the technicians completely at arm's length.

So that, where they used to stand and fiddle with the machine and easily make changes, now they must stand back and work by remote control and in a much more difficult manner, so that all changes become enormously more expensive than they would have been to make in the unactivated machine.

Therefore, when you first put your first charge of deuterium-tritium into the machine, you will wish to know just exactly what to expect. And, if ever you say: "Ha, look at this! Isn't this surprising?" Then, that is very bad news, because you have then endangered the whole investment in the machine. Preparatory steps, precautions that you could have taken, which would have been little trouble, were not taken. And now you have a real problem.

So, in this sense, it is really most cost-effective to proceed in a step-wise manner. Do one machine at a time, stepping up the amount of burning as you go. And, so, that is a large part of the rationale for the Compact Ignition Tokamak, the CIT.

It's mission is not to be reactor-like in the engineering sense, like the ITER. It is to be completely reactor-like in a physics sense. To produce real burning plasma, just like that in the ITER. To have a flexible experimental apparatus, so one can try the full range of techniques that will be needed in an ITER or other engineering test reactor. To have elaborate diagnostics, so that you can learn the lessons. And then to pass that on, so that, when the ITER goes into operation, we will have knowledge and experience rather than sudden surprises.

The next question was what about the capability of the CIT to ignite. Before answering that, one should ask about the capability of the community to make predictions.

And, on that score, I would say we have empirical scaling laws which are extremely good in giving a lower-bound on performance. These scaling laws have held up in a wide variety of experiments. And whenever nothing special was done to get good confinement, that has been described by these lower-bound scaling laws.

The second piece of information is that in experiments where one has had good control and where one has applied lessons learned from basic plasma physics on how to optimize, one has improved confinement by factors of two or three, and greatly enhanced the potential for D–T burning.

So, as we look to CIT, we wish to have both these elements. We wish to have a very high lower limit on performance, which mainly means brute parameters like magnetic field strength and current. And those we have made very strong, much stronger than in any existing machine, and comparable to what is being planned in the ITER.

Second, we wish to incorporate the capability to get these enhancements in confinement, and we have done everything that is known today to make that possible. If the situation is that if the CIT has the enhancement in confinement, then it should be able to reach ignition, so to speak, with great ease and with a very large operating window.

If the confinement is poor, then, when it's pressed down against the unenhanced confinement, but one can still scrape through. So,

the margin, in this sense, is tight, but it is made broad if we can achieve the capabilities for enhancing the confinement.

And that is really what we want to do. Because, when we go to the ITER stage, it is bad news for them if very huge crude parameters are needed, such as very large fields and currents. And it is much better news for them if we have learned how to operate the machine so as to get enhanced confinement with more moderate basic requirements, because that will greatly reduce the cost of operating and building that machine.

Okay. So, that, then, explains why it is useful to do the CIT. The next question is, "But, then, why do we need TFTR and the base program?" And the answer is exactly the same.

By doing TFTR and having the base program, then we minimize surprises as the CIT enters D–T operation. CIT needs to have a very long run in D–T, and that is why one needs to know ahead of time what sort of physics will be done and what sort of operational problems will be encountered.

So, what can I say about our near-term deuterium-tritium capability that exists or will exist in three machines: in TFTR in the United States, in JET in Europe, and in the T–14 device, which is just being completed in the Soviet Union in the vicinity of Moscow.

At the moment, TFTR is ahead in temperature. It is ahead in fusion power. It is closest to break-even. It is now already within a factor of five or even four of the break-even point in deuterium-tritium plasma.

Just making simple projections from what we have, this summer we hope to come to a deuterium regime which will be within a factor of two of the D–T break-even. And, as to what our prediction is for getting to break-even, it is exactly the same as for CIT.

That is, the known empirical rules give us a good lower limit on performance which we can trust. That lower limit is within a factor of two of break-even. Improvements in confinement should easily take us to break-even.

Those you have to see, but we have demonstrated improvements of this order in TFTR. And there is no reason to think we cannot do it in the break-even regime.

So, my view is that break-even will be reached, and that is our goal.

Aside from reaching break-even, we hope to learn a good deal about the physics of burning plasmas in TFTR and I should say, in that sense, that how far we are on the scale from break-even to ignition is not everything.

There are special features to TFTR namely, very high temperature and moderate density, which lend themselves to making exceptionally large populations of the alpha-particles which are the heart and soul of the burning plasma.

So, in terms of that aspect, the study of the alpha particle burning plasma physics, the TFTR is a match for many of the ignition experiments, which do not produce a greater alpha population than this. So, we hope to learn a lot.

The state of progress toward the D–T phase, TFTR has identified the operating mode which it plans to use in the break-even experiments. All the systems that will be used are now in place.

As has been mentioned, tritium will begin to be introduced in 1988; the full tritium system ready in 1989. D–T experiments will begin in 1990. And the maximum performance experiments will be done in the first part of 1991.

JET is a larger machine. It is more complex. It is still looking for its best operating mode. It is still adding new systems.

The schedule calls for them to do their D–T experiments in 1992. The number of things that still have to be done leads us to hope that they will have time until 1994 in order to complete their check-out of the new systems and actually go into deuterium-tritium operation.

The T–14 in the Soviet Union is similar to TFTR. It is smaller, simpler. They may be on a fast track. They are planning to start plasma operation at the end of the year, and they will have a few years to go, I think, before they enter into D–T operation.

Now, on the topic of international relationships among these projects, there are always two ways to tackle this. One of them is in terms of competition, and the other in terms of cooperation. On the competitive side, the key fact is that the United States is in the lead.

One amusing thing that one might speculate about is: if the Soviets had the TFTR and at this point were announcing they were close to break-even and about to put in tritium, then how would the U.S. community react.

And I think the way they would react is that they would come right to you and say: "The sky is falling. The Soviets are way ahead of us. Please help us catch up by giving us more money."

So, that's what one would say on the competitive side. On the cooperative side, the plan is for people from JET and——

Is that a signal for me to stop almost immediately, or is that just a coincidence? [Laughter.]

I will be through in just a second. Is that all right?

Mrs. LLOYD. Please proceed. We've just gone in session. If two bells ring, we have to walk out of here. But one bell means you can keep talking.

Dr. FURTH. Could you give me a clue how many more minutes I may be allowed to take?

Mrs. LLOYD. You can go ahead. If two bells ring, we have to go vote. All right? So, just go ahead.

Dr. FURTH. Okay. I will try to finish quickly. Very good.

Now, on the cooperative side, in the period 1989 to 1991 when we will be doing our deuterium-tritium start-up experiments and break-even experiment, many other people have indicated a great interest in visiting the Laboratory and, in this way, getting their first instruction on how such things run so that they can apply it in their experiments, and then they will be able to go further.

After 1991, the Laboratory staff has been invited to visit other places such as JET to help them do their thing and to benefit from what they have done. And, for the period after 1996, if we succeed in getting CIT on the air by then, the other nations have indicated their great interest in coming to visit CIT and participating in CIT.

You have already heard that the ITER will represent major international collaboration when it goes on. But, the actual experimental runs, most likely, will be after the year 2000.

So, I think I have answered why CIT and why TFTR are worried about the base program. One way to say it in the crudest terms is the elements of the base program are the seeing-eye dogs of the program.

The small experiments have provided essentially all the clues which the big experiments have used in achieving success and in getting these enhancements of performance, which make the whole thing look good, and make the prospects good for achieving break-even in ignition without going to extreme parameters.

That is all developed by small experiments; that is all digested and explained by theorists; and that is really what we count on. Without it, we would be literally blind.

Theory is incomplete, as I said, but I don't conclude from this that we need less effort on theory; I conclude we need more effort on theory so that we can get this important job done.

In regard to alternate concepts, I would also like to say, we don't understand the theory so well that we can declare the tokamak to be the clear winner. There is a great deal to be said for alternate concepts such as the stellerator and the reversed-field pinch. They still have good reactor prospects, good properties of their own, and they contribute invaluable physics, the really profound physics insights again which have use to all of us.

And finally, technology contributes all the basic tools we now use and is working on the important tools for the future.

A quick comment on the fusion budget for 1989. It is tight, very tight, but workable for the base program and for TFTR, in my opinion. For CIT, it means that we can continue to get a good start. We can complete the preliminary design, we can do certain full scale prototypes, and that means that, in 1990, we can enter into component fabrication and site construction with a view to completion of the project in January of 1996.

Let me also comment on the DOE flat budget, hypothetical case proposed for fiscal year 1990 to 1993. In a way, we appreciate this very much because it shows how much the department appreciates the central role of TFTR and CIT.

Also, we think it is a productive exercise in the sense that it shows that if you can squeeze CIT even into a flat budget, then really there can't be some huge sum missing in order to finance the transition from TFTR to CIT.

In fact, it is something like ten percent of the annual money coming into the program would affect that transition without having to destroy any present program element, and without having to squeeze the base program. So it shows this is not an enormous problem.

If we actually were to implement this flat budget case, it would force a stretch-out of CIT, which is unnecessary and undesirable, and it would greatly disrupt the base program, including disrupting some things at Princeton which we think are very productive like the PBX-M experiment. So, we hope very much this will not happen.

The realistic approach will be, I think, to adopt the President's 1989 budget, and then to have a gentle but firm upward growth of the budget, and in that way, we can meet our important break

even in ignition objectives, and also bring along the base program which we need to make a success of our work.

So, this Committee has always been very enlightened and helpful to us in the past. I hope very much that you can help to make this happen.

Thank you for listening.

[The prepared statement of Dr. Furth follows:]

Statement of Harold P. Furth
Director, Princeton Plasma Physics Laboratory
before the
Subcommittee on Energy Research and Development
House Committee on Science, Space, and Technology
30 March 1988

Madame Chairman, Members of the Committee:

Since the initiation of the Tokamak Fusion Test Reactor (TFTR) project, in 1975, the fusion program has made steady progress toward the understanding of hot-plasma confinement and the production of useful energy. One way we measure our progress is by comparing the fusion power that is generated in a hot plasma with the power that is required to heat the plasma:* The experiments of the mid-70's still had a factor of a thousand or more to go towards the achievement of break-even conditions, where the fusion power equals the plasma-heating power. As of last year's TFTR run, there is less than a factor of five to go. I find it particularly encouraging that last year's TFTR results were achieved using only a fraction of the full TFTR plasma current and a fraction of the plasma-heating power that is now available. During this year's TFTR run, we hope to advance to within a factor of two of break-even conditions.

The Laboratory's plan is to introduce tritium onto the TFTR site this fall, have the tritium system fully operational by next fall, and carry out the D-T break-even demonstration during '90-'91. After '91, the TFTR experiment will be shut down to make available funds and facilities for the Compact Ignition Tokamak (CIT) experiment.

*Performance is usually calculated in terms of the reactor-relevant deuterium-tritium (D-T) fusion reaction, but pure deuterium or hydrogen has served as a substitute for D-T in plasma-confinement experiments to date.

The TFTR-CIT-ITER Sequence

As fusion research progresses from the study of ordinary plasmas to the study of burning D-T plasmas, scientists will be confronted with a number of new plasma phenomena as well as new technological challenges. In D-T operation, plasma-confining experimental devices become radioactive -- essentially on the first successful shot. While the amount of radioactivity is relatively small, it is sufficient so that remote handling and other special experimental operating techniques must be introduced.

The CIT experiment will provide necessary experience with the handling of burning plasmas for the benefit of reactor experiments of the future, such as the proposed International Thermonuclear Experimental Reactor (ITER). On the basis of the CIT experience, it will be possible to make well-formulated plans for the reactor operating phase prior to the activation of the machine -- thus safeguarding major investments. In the same way, successful entry of the CIT project into its burning-plasma phase will build on the experimental data and the practical operating experience gained in the D-T phase of the TFTR project. (The somewhat later D-T-experiments on the Joint European Torus [JET] would benefit similarly from the TFTR experience and would be valuable in strengthening the CIT data base.)

The new plasma-physics phenomena expected in burning plasmas are mainly due to the presence of alpha particles -- the energetic helium nuclei that are produced by D-T fusion reactions. The moderately dense, very-hot-ion plasma regimes that have been achieved in TFTR are particularly well suited to produce significant alpha-particle populations. The special diagnostics,

neutron-shielding, and maintenance techniques being developed for D-T operation in TFTR are similar to those needed for CIT. The tritium-handling technology will also be the same: We plan to reuse the actual TFTR tritium systems for the CIT experiments.

The CIT design takes advantage of the last ten years of progress in the physics and technology of tokamak confinement to raise performance from the break-even level to the ignition level. The ignition capability of the CIT is based on two principal factors: (1) powerful machine parameters, such as high plasma current and magnetic field strength and (2) refined plasma-control techniques, such as special shaping of the plasma density profile and use of a magnetic divertor at the plasma edge.

The maximum attainable CIT plasma current and magnetic-field strength are several times higher than the currents and fields of the most powerful present-day tokamak devices. This combination of machine parameters gives the CIT a fundamentally superior plasma-confinement potential -- comparable to that proposed for engineering test reactors in recent design studies. In addition, the CIT design emphasizes the use of special plasma-control techniques to enhance the quality of confinement, because qualitative improvements will be particulary useful in minimizing the future development cost of tokamak reactors. The large parameter range and flexible experimental capabilities planned for the CIT will make it the preeminent research facility of the world fusion program during the latter part of the 1990's.

At present there is a spectrum of scientific opinion about how large an improvement factor in the quality of tokamak confinement will ultimately be achieved by special techniques. To help take both optimistic and pessimistic expectations into account, we are developing a flexible project strategy. The CIT experiments will begin at a plasma current and magnetic field level sufficient to reach ignition under favorable confinement conditions that are being achieved in present-day tokamak devices prototypical of CIT. This operating mode will utilize the available TFTR power supplies. Depending on the trend of confinement-physics results, the machine parameters can be raised toward their maximum values by increasing the input power to the magnet system.

The CIT conceptual design has been developed by a national team, led by PPPL. The design process has been guided by the Ignition Technical Oversight Committee (ITOC), which consists of distinguished U.S. fusion scientists and engineers.

CIT Plans for FY'88 and '89

In January 1988, the national CIT team completed a conceptual design study that integrates CIT plasma-physics models with the engineering of the machine components and the ancillary systems and facilities. The physics concepts and the engineering design of the individual CIT systems are now supported by extensive detailed analyses.

During the conceptual design study activity of the past year, the plasma major radius was increased, in order to maximize the physics and engineering margins of the basic CIT device. The change in size has had a limited impact on the design analyses, since the relative scale of the machine components has remained about the same.

Extensive R&D has been carried out, particularly on magnet-coil materials. The development of principal machine components will be completed this year, and the project will proceed to full-scale prototyping and manufacturing studies. A complete logic network has been established for linking the tokamak component-development activities to the final machine design, as well as to the component-fabrication and assembly plans. During the present fiscal year, the preliminary design phase will begin and the engineering support contracts will be awarded.

The FY'89 project activities include the completion of preliminary (Title I) design of all major systems and the start of A&E design for the CIT buildings. Full-scale single-turn prototypes will be produced for the toroidal-field coil system. Full-scale poloidal-field-coil prototypes will also be produced, and a coil-testing program will be initiated, with completion scheduled for FY'90.

The activities planned for FY'88 and FY'89 will create the basis for initiation of the component-fabrication and site-construction phase of the CIT project in FY'90.

The Magnetic Fusion Base Program

While my presentation responds primarily to the Committee's questions concerning the burning-plasma aspect of fusion research, I would like to emphasize the crucial role that the development of basic fusion science and technology continues to play in support of our advance towards fusion energy. Our major operating fusion devices owe much of their success to insights derived from plasma theory and smaller experiments. The direct dependence of the CIT design on theoretical understanding and innovative experimental techniques has been brought out in the previous discussion.

I would like to note, furthermore, that important experimental advances are being achieved not only by tokamaks, but also by other confinement schemes -- notably the reversed-field pinch and the stellarator. There is a strong case for the continuing exploration of these concepts alongside the tokamak, as well as for the pursuit of the theoretically indicated opportunities to optimize the tokamak concept itself. A well-balanced fusion program should include substantial efforts along all these lines.

MFE Budget for FY'89

The President's FY'89 budget of $360M supports the most essential steps needed to advance towards the TFTR break-even objective and the construction of the CIT, while maintaining a vigorous base program.

Some significant negative impacts of the FY'89 budget are: (1) TFTR experimental approaches to break-even are being narrowed -- for example, we have lost support for construction at ORNL of a tritium-pellet injector. (2) The CIT construction schedule is being stretched out.

Projected MFE Budgets for FY'90-93

In response to a congressional request, OFE has prepared a budget study that shows how the construction of CIT could be accommodated within a set of MFE budgets that remain constant in '89 dollars (escalating to $375M in FY'90 and to $426M in FY'93). To fit within this difficult budgetary constraint, the CIT construction schedule is stretched out, at the cost of opening a five-year gap in frontline fusion research between the shut-down of TFTR in '91 and the start-up of CIT in '96. The incremental funding that is needed to make the transition from TFTR to CIT is generated by "taxing" the balance of the MFE program: there is a purely funding-driven two-year hiatus in the highly productive research program of Princeton's PBX-M device (during FY'90-91); painful constraints are imposed on other innovative experimental programs; and there is a general dearth of new initiatives, aside from the CIT project itself.

The DOE budget study is important and welcome in that it makes a clear statement concerning the central role of the CIT project within the fusion program. The five-year "flat-budget" illustration is also helpful in demonstrating that no really major capital-construction supplement is needed to help us make the transition from TFTR to CIT. From the point of view of

maintaining the vitality and productivity ~~of the~~ US fusion community, however, an actual policy of trying to raise the entire CIT construction cost from within the existing program would be quite problematical. Such an approach tends to subject the CIT construction schedule and the MFE operating budgets to levels of pressure that seem to go beyond the point of cost-effectiveness.

Is MFE an Energy Program?

In planning the future of U.S. fusion research, one basic question is whether fusion should be thought of as a science program or as an energy program. I would like to conclude my remarks by addressing that question.

During the first decade of fusion research, a major program goal was to produce at least some measurable fusion energy release. This goal was reached at the end of the 1950's, with the production of about a million fusion reactions per experimental "shot". That corresponds to an energy release of about one millionth of a joule -- or equivalently, the work done by a horse in about one nanosecond (one billionth of a second). During this same era of fusion research, the field of modern plasma physics was founded and most of the basic discoveries and inventions of plasma-confinement theory were made -- but one may well hesitate to describe this fruitful activity as an "energy program".

Last year, TFTR produced two times 10^{16} fusion reactions per shot in deuterium plasma -- which corresponds to a horse working for twenty seconds. If D-T fuel were introduced right now, the horse would be working for more than an

hour. When TFTR reaches break-even in D-T plasma, the energy released during each one-second shot will correspond to fifteen horses working for an hour. I would call that at least the beginning of an energy program.

The CIT will release about one gigajoule of fusion energy during a five-second burn, which is equivalent to five hundred horses working for an hour. The reactor objective of the ITER corresponds to about one million horses working a good part of the time. As you consider all those horses hard at work, I hope you will agree with me that fusion research is at least a bit of an energy program right now, and has the opportunity to become much more of an energy program in the fairly near term.

I should like to express my appreciation to the Committee for its confidence in the future of magnetic fusion, and particularly for its far-sighted support of the TFTR and CIT projects. Adoption of the President's FY'89 budget, along with modestly rising funding during the next few years, will permit us to make a significant start on the burning-plasma phase of fusion research. With the help and encouragement of the Congress, the magnetic fusion program can continue to evolve into a powerful and economically relevant energy program, while remaining a first-rate science program as well.

DISCUSSION

Mrs. LLOYD. Thank you very much for your detailed testimony. It is very good. A question I asked Dr. Clarke, review of the CIT in January by the external group indicated concerns about the design in terms of the changes to achieve the plasma, and obviously you modified this design, based on this review. We'd like to know the changes and the technical implication of these changes, Dr. Furth.

Dr. FURTH. There was a spectrum of views expressed, ranging from the Russian view, which was that it would ignite maybe at less than even peak parameters, but that they thought our engineering was too cramped and they kept saying don't make the engineering so clever, make it a little more stupid, make it a little bigger, so that it will be flexible.

And then, at the other end of the spectrum, people who believe only a very, very large machine can ignite, and from this concludes our machine is too small.

What we did was to make the machine about 20 percent bigger, which is just about what the Russians were suggesting. In this way, we were able to raise the current and the magnetic field, and when I checked with the Chairman of this review committee, Dr. Callen of Wisconsin, he looked at this and thought this was pretty much in the mainstream of the sort of change that the Committee would be pleased to see.

How we were able to do this without raising the cost of the project? As Dr. Clarke explained to you, it is done by initially putting the machine into operation in such a way that it will ignite under the assumption that you get a big enhancement factor—you know, the enhancement factor which I mentioned due to special techniques, it assumes that that will work fully and if that is true, then with initial power supplies now available in TFTR, the machine will ignite.

If that is not true, then one must add power supplies to bring up the machine parameters to full performance. But now full performance is higher than it ever was, and so, the margin of assurance for success is greater.

Mrs. LLOYD. Of course, this Committee is more interested in success than we are trying to keep—we all operate in terms of budgetary constraints, but our goal is a successful program and I hope that you understand that.

If you had additional funds, however, what are the changes in the design at CIT would you like to make such as heaters, or operations that you would like to see come into place to achieve, or burn, at an early as possible date?

Dr. FURTH. At the moment, I would not further increase the parameters because, as I mentioned, the most useful, most valuable, most successful thing we can do is to do it skillfully, rather than by brute force, because that will be of maximum use to the reactors that come after us.

And therefore, if I had money, I would put it, for example, in rapid development of electron-cyclotron heating type frequencies which might be——

Mrs. LLOYD. This is what you would do with additional money?

Dr. FURTH. Yes. It might be a kind of magic tool for putting the heat exactly in the right place while disturbing the plasma as little as possible. And, past experience has shown that the sort of improvements you can make in confinement with clever tools correspond to things like doubling the plasmas current. So, the leverage of having a really good tool is far greater, really, than the leverage of the gross parameters. Of course, you need certain minimum gross parameters to be in the right ballpark.

Mrs. LLOYD. How much has been invested in the capability of the TFTR to operate with D–T?

Dr. FURTH. I think, as we are standing now, Dr. Meade is the expert—but $317 million has been spent on——

Mrs. LLOYD. How much more remains to be invested?

Dr. FURTH. $56 million.

Mrs. LLOYD. 56, all right. Well, what is the operating cost of TFTR relative to that of the other large Tokamaks such as JET and JT–60? If you compare their operations and their accomplishments?

Dr. FURTH. Maybe I should give Dr. Meade a chance——

Mrs. LLOYD. That seems like a hard one, I would, too.

Dr. MEADE. Thank you very much for the opportunity to be here today. In this particular regard, the TFTR operating budget is now, for 1989, $74 million. The JET budget, and I was just at the JET project about a month ago, is now $120 million, and has been at a level in the range of 50 percent higher than TFTR for many years.

The Japanese Tokamak, JT-60, has an equivalent budget of about $160 million and again has been at that level for several years.

These budget levels at JT-60 and JET have allowed those projects to add more systems, they have more flexibility——

Mrs. LLOYD. Also, there's a larger—it's larger. Does that affect—is that a factor in the increased costs?

Dr. MEADE. The system itself, and if you visit the sites, you will see that the systems are significantly larger physically than TFTR. However, if we now address the present plasma physics accomplishments, as was noted in an earlier testimony, the TFTR has the world record for the highest plasma temperature.

It also, in separate experiments, has the world record for the highest n tau, produced with very high density plasmas. We have also the highest value for the effective break even parameter, the Q, the energy multiplier, being at one quarter of that of break even at the moment; roughly, twice that that JET has accomplished with their present equipment.

Earlier testimony had indicated that JET has greater inherent capability in the long run. However, TFTR, by carving out a special niche of the high temperature plasmas, has been able to achieve greater an infusion production at the moment.

And, I would add that that was a key part of the TFTR project in 1974, when it was conceived. It's always been a central theme that we will use this very special regime to enhance the fusion reactions by creating this so-called beam target reaction. That still, now 13 years later, is our theme. We've been able to maintain that technique throughout our entire project.

Mrs. LLOYD. What is projected for JET—do you anticipate they will try to go further than TFTR?

Dr. MEADE. Yes, they will. Their program, as I indicated, has been adding a number of auxillary systems that we will not have on TFTR. I anticipate that to get maximum use of these auxillary systems, they will require additional experimental time and, hence, their actual D-T experiments will be probably one to two years, at least, behind TFTR's D–T experiments.

We are planning our experiments to start to mid-1990's and finish in 1991. And the JET experiments, I would imagine, would be in the 1992 to 1993 timeframe.

Mrs. LLOYD. Good. Are you planning to send any of your technical personnel overseas to the JET or JT-60?

Dr. MEADE. Yes. The meeting that I was at, at JET a month ago, one of the discussion items was that after TFTR had completed its D-T program, what we want to do is send a significant number of our staff to the JET project in the 1992–1993 timeframe, to participate in their D-T experiments and then to return to CIT in the 1996 timeframe, to carry on these experiments on CIT.

We have also invited the Japanese and the Europeans to participate in the first TFTR experiments in the mid-1990's, and so there's quite a nice flow of personnel back and forth from these projects.

Mrs. LLOYD. How many people are you planning to send over to work on the project?

Dr. MEADE. Roughly, half of our research staff will be going on these research leaves. I anticipate sending 15 to the JET project, 10 to the JT-60 project in Japan, and probably another 5 to other projects within the United States, that will still be interesting projects going on in the United States.

Mrs. LLOYD. Very good. I'd like to go back to the tritium experiments for TFTR. To what extent have you briefed the surrounding community, including the university, on the experiment? We don't want any surprises in this area, as well, Dr. Furth, since you mentioned surprises.

Dr. FURTH. Yes. We have briefed them quite extensively. The University has had some of its very specialized professors in this area looking at us. The community has been brought in. We have spoken to their governing bodies. We have given endless tours to people in the vicinity. There are literally 5,000 people a year who come through, to all of whom we explain with great care what we are about to do.

Mrs. LLOYD. What is the degree of acceptance?

Dr. FURTH. At the moment, there has not been the slightest sign of non-acceptance. There has been a great deal of community interest.

Of course, the definitive thing on these matters are the environmental assessment that will be carried out for CIT. Similarly, formal processes, of course, have long ago been started for our TFTR D-T, back in the 1970's, and coming along step by step, and they're being updated now.

So, the whole thing is being handled in a very formal, orderly and public fashion.

Mrs. LLOYD. Very good. That's very good news. How much of your current budget has been allocated to the decontamination of TFTR?

Dr. FURTH. Dr. Meade, perfect for you.

Dr. MEADE. We have been preparing our budgets for the 1990 timeframe. The decontamination plan for TFTR is as follows. We will stop operation in mid-1991 and then we will secure the TFTR test cell area for approximately four to five years, let the radioactivity decay, to a level at which we can work on it without requiring extensive remote handling type of equipment. This also fits well with our present plans for CIT, in that during that period of time, CIT will be finishing its fabrication phase.

In kind of the mid-1990's, we would then begin the decommissioning of the TFTR. What we will do is the Tokamak itself will be cut into pieces. We have an agreement with Hanford to ship it to the Hanford site. I need to give you a magnitude of the amount of radioactive waste that will be left at this period of time.

The actual activity of TFTR will be 500 curies, to give you a feeling for what that means is they are off in these exit signs on airplanes that have tritium in them, and they have in the range of 25 curies. The activity is really a very modest amount. It is something like 20 of these signs.

When we take it to Hanford, it will be classified as Class A waste. It is in fact one percent of the maximum level allowed for Class A waste.

Mrs. LLOYD. Very good. Speaking in this timeframe, my final question is what will be the impact on the PBX-M program, on the proposed shutdown?

Dr. FURTH. Maybe I could pick that up again. First, what does the PBX-M program do? It pursues making very high pressure plasmas such as one would want to have in a practical reactor to maximize the fusion power. As Dr. Clarke mentioned in his testimony, there is some prospect that we could leap far beyond where plasma pressures have been limited thus far into a new regime, if only we mastered the trick. PBX is meant to master precisely that trick by means of its special been shape.

The progress between now and the shutdown is expected to be substantial, but needless to say, being shut down for two years is no help. That interval will be used to perfect new systems, which will then help us when the machine comes back up.

Clearly, this would be a disruption in a very valuable program.

Mrs. LLOYD. Mr. Morrison?

Mr. MORRISON. Thank you. It is good to get word that at least some part of the fusion program is coming to Hanford. [Laughter.]

Even if to be buried. I know that throughout the work on fusion, and my question will be rather generic, there have been some tough decisions as to which technology paths to follow. I am just wondering if in your contact with your colleagues, what is happening in other technology areas, reverse field pinch, stellarator, just out of information, can you share that with me?

Dr. FURTH. Yes. They are all making quite interesting progress. I have mentioned the reversed field pinch, which from an experimental point of view, has produced quite interesting parameters in recent years and it is now their endeavor to raise their plasma current to the level of millions of amperes where things should get even more exciting. That requires construction of new machines. That is what they are about at the moment.

On the stellerator side, there has just been completed and put into operation a new device at Oak Ridge and we are just about to start on a productive career there.

Aside from that, there are experiments on compact toroids of various kinds being pursued on a small scale and beyond that, one gets to the still smaller endeavors and theory of other good things one might do.

Mr. MORRISON. Are we pursuing the right path?

Dr. FURTH. For the moment, to pursue the Tokamak is clearly a very good idea, because that is what takes us to the relevant regimes and that is really the only way, in the near term, that we will be able to do experiments with burning plasmas. As to what the right path is ultimately, one cannot say. Indeed, optimism suggests that one hopes there will be better and better right paths down the ages.

Mr. MORRISON. What are other countries doing? You made some mention in your testimony. Are they pursuing the wide range of things that will ultimately help us all get to the goal?

Dr. FURTH. They are actually pursuing a somewhat similar range, that is strong emphasis on having front-running Tokamak in the TFTR class, then alongside that, several Tokamaks to develop new Tokamak techniques, advanced innovative Tokamak techniques and then there is a tendency to have stellarators and reverse field pinches and smaller original ideas alongside that. More or less the same judgment has been made about what is important and productive.

Mr. MORRISON. As I recall, maybe Congressman McCormick can correct me, I think initially you had built into your long range planning about $1 billion a year. I look at the numbers and I am impressed with how far we have come. I wonder how far we might be if in fact we had been able to stay with that sort of an ambitious schedule. That is fun to speculate about during these times when we almost have to stay with level funding or even cutbacks.

I think probably the budget as proposed by the Administration is a tribute to those of you who work in this field.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you, Mr. Morrison. Thank you, gentlemen, for your testimony. It is always good to see you.

Our next witness is Professor Bruno Coppi from Massachusetts Institute of Technology. We welcome you to our hearing and we look forward to your testimony. We do have your complete testimony and you may summarize or proceed as you wish. We just look forward to hearing from you, Professor.

STATEMENT OF DR. BRUNO COPPI, PROFESSOR, DEPARTMENT OF PHYSICS, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, CAMBRIDGE, MA

Dr. COPPI. Madam Chairman, Members of the Committee, I wish to thank you for taking the time to listen to my views on the long and short range course of the U.S. program concerning the feasibility of experimental fusion reactors.

As you probably know, since the summer of 1975, I have been involved with proposing and designing experiments that have the

purpose of proving that a plasma mixture of deuterium and tritium can ignite by fusion reactions.

In fact, the Alcator experiments of MIT had shown by that time, 1975, that existing technologies together with newly available information about the physics of magnetically confined plasmas could be used to initiate the construction of an ignition experiment. The proposed experiment was called Ignitor and like Alcator had compact dimensions, relatively high magnetic fields and could operate with relatively high particle densities. Under these conditions, we already knew that the expected confinement properties were good and that the plasma had a high degree of purity, a characteristic necessary for fusion burning regimes to be reached.

Since many of us have been waiting for nearly 13 years to see that an ignition experiment, and in particular one of the compact type as originally proposed, could be finally undertaken in the United States, I am of course anxious that we do not have to wait another decade or more before an experiment of this type is brought to significant operation. The rest of my testimony follows somewhat from the statement.

I think the world needs to know whether a fusion reactor is feasible. Developing countries expressed very eloquently their point of view in a meeting in Rio de Janeiro last summer, and they are particularly sensitive about this point.

With all the constraints imposed by the environment, how can we satisfy the world's energy needs if the average per capita consumption was to become comparable to the levels of the most affluent countries? The affluent societies are sustained by the bountiful and inexpensive energy that is provided by fossil fuels. Shortages of these fuels, either actual or for environmental reasons, are not inconceivable in the foreseeable future. You could in fact refer to the New York Times of yesterday.

I would like to stress that the U.S. has a unique pool of talent and a well established tradition of being able to carry out original and outstanding scientific enterprises within relatively short time spans. I think this ability is sorely needed in the worldwide effort to produce a working fusion burner.

In fact, it is my belief that the U.S. fusion program in spite of the successes, past successes that we heard about today, is now very close to entering a state of hibernation. This involves the questionable deuterium-tritium operation of the TFTR machine at the Princeton Plasma Physics Laboratory and the consequent rapid demise of this machine resulting from the level of radioactivity induced in it; the slow construction and the late beginning of tritium operation of a large size and probably marginal, in terms of achieving full ignition conditions, CIT device; the looming doubt that the considerable quantities of tritium needed for a meaningful series of experiments by the presently proposed CIT and the problems associated with these experiments may not prove acceptable to the residents of the Princeton neighborhood; the prolonged idling of the outstanding experimental staff of the Laboratory.

At the same time, most other groups, even with the FY 1989 budget, in various academic and industrial institutions associated with fusion research continue on their way to extinction and emargination.

What are the recommendations? The experimental test of ignition is an unusually well-justified scientific undertaking that is long overdue. We currently lack any experimental knowledge of the physical conditions of plasmas that are effectively heated by the particles produced in a fusion reaction.

In addition, I should say that the line of experiments represented by Alcator, that has been both cost effective and scientifically successful, Ignitor, that has received initial funding in Europe, and a Compact Ignition Tokamak are very promising. It could in fact teach us how to burn more desirable fuels than deuterium-tritium, in particular the deuterium-helium 3 mixture, where the prevalent reaction does not produce neutrons. That would be the true "clean" fusion.

Under the present schedule, experiments to approach ignition conditions may not begin until 1998 or later. I believe that this is much too long to wait. Therefore, I would like to see a greater sense of urgency in proceeding toward the construction of a compact ignition experiment, with both the technical and management effort being devoted to finding the most expeditious solutions that would allow meaningful experiments on deuterium-tritium reacting plasmas within the first half of the next decade.

I have said this because, as I said, in the U.S. we have this long tradition of being able to operate fast.

Therefore, since it is evident that in the absence of new funding, the proposed tritium experiments at the TFTR machine of Princeton will compete with a reasonably rapid construction effort for a Compact Ignition Tokamak, and I call that "CIT," not the present one, and I would recommend that the tritium experiments be postponed indefinitely. This postponement would free up significant funds that could be used to increase the construction pace of a CIT.

In addition to the possible conflict with a concurrent CIT project, there are other considerations which suggest that the tritium operation of TFTR not be performed. Among these are the possibilities of achieving the so-called breakeven $Q=1$ condition in TFTR, that looks very uncertain at this time. It could be demonstrated more effectively in a compact machine. The value of achieving an improbable breakeven TFTR is certainly not commensurate with the costs incurred by rendering the device radioactive and consequently losing a very valuable facility to further operation.

We should remember that this machine has required a large investment in both time and resources and severe sacrifices from the rest of the interested scientific community.

The number of discharges that could be produced with deuterium-tritium mixtures at full power in TFTR is very limited. This would obviously hinder the execution of any substantive program of physics investigation.

If the tritium experimental phase of TFTR is not pursued, extended operation of other PPPL programs of crucial importance to the development of efficient fusion reactors would be assured. As an example, the permanent mothballing of the PBX-M machine could be avoided. A well-conceived series of investigations could deal for instance with a number of interesting problems that I have listed here, and let me say that the solution, this solution would also avoid the danger of idling or more likely losing the best

members of the Princeton scientific staff, which is an inevitable outcome given the large and uncertain gap between the shutdown of TFTR and the actual operation of the presently proposed CIT.

Therefore, it is essential that the funding freed by such a decision not be completely withdrawn from the PPPL program and that part of it should be used to exploit the existing PPPL facilities to their full potential, with deuterium plasmas only, in order to obtain vital physics information needed to improve the performance of future experiments.

The second part of my testimony refers to an alternative scenario to the fusion program and this is an answer to a precise question of our Chairman. You may consider this to be radical, but I frankly believe that this is kind of logical and I can tell you that there is large support around the country for this type of solution.

Since the letter of the Chairman to request my opinion about the possible alternatives to the present chartered fusion program for the U.S., I shall dwell on possible radical alternatives.

Speaking bluntly, I think that with a judicious addition of funding, and I was delighted to hear that you are willing to consider a 10-percent increase, to the existing five-year plan, this program could be converted from one where the U.S. effort hibernates until the year 2000 but as we say, later, it will not wake up to a spring, it will wake up to a situation where a great deal of the presently existing knowledge will not be there and probably beyond, to one that has current impetus and a vigorous future.

An "upbeat" option would first of all choose a site that is already approved for the operation of nuclear reactors, with containment buildings available and the immediate access to a node of an electrical grid that can serve as the power supply for the foreseeable generation of experiments.

Several such sites, that are needed to ensure the nuclear aspects of the U.S. fusion program a healthy evolution, exist.

This site will house our advanced ignition experiment, I call it AIX, that will have a higher probability to reach deuterium ignition, and will also try to burn a deuterium-helium 3 mixture.

Now, about international cooperation, concerning the Fermilab of Batavia, Illinois, as a model, the new site would be operated as a truly national facility and become part of the worldwide network of the few sites that are beginning to be established in Europe, USSR, and Japan for the development of near-term experimental fusion reactors.

That is, all of these other countries have either chosen or identified one or two sites where you can truly do, without restriction of available power, environmental problems and so on, the deuterium-tritium experiment we need to carry out in the foreseeable future.

The resources, human and material, that the existing program allocates to ITER, for the sake of international cooperation, should be devoted to nearer-term fusion burn experiments that are truly likely to be constructed, rather than to remain an exercise on paper.

Given the extensive high-level acquaintances I have in Europe and the Soviet Union, it is my firm opinion that experiments on such a site, if expeditious and well-conceived, will attract their participation.

The history of the Alcator program at MIT is proof of this. They all came to work with us. In fact, significant participation on a near-term fusion burn experiment would also lay the ground work for a more extended and thorough large international cooperation.

Also, U.S. university and industrial groups should be encouraged and funded to contribute as active partners, again, as in the case of elementary particle physics.

Within this scenario, it is important that the Princeton Plasma Physics Laboratory remain a strong international center for plasma physics devoted to the theoretical and experimental study of the fundamental physical processes relevant to fusion research. I have a stake in that since I grew up at Princeton myself before going to MIT.

The TFTR facility would be assured an adequate life that, scientifically speaking, would provide an appropriate return for the investment made in it.

The present CIT effort would be converted into a CPT, Compact Princeton Torus, at substantially lower costs and faster schedule, as it would no longer be burdened by the problem of being limited in its operation and accessibility by the level of machine radioactivity associated with tritium operation.

It would be sized to fit within the existing test cell and the present, already large, electrical power supply system, but would adopt innovative technological solutions allowing it to attain new and exciting physical regimes.

Thus, the outstanding expertise in plasma physics that now exists at Princeton would be utilized at its best.

In summary, the U.S. is at a critical point on the road to demonstrating that fusion can be a real and possible energy source. It can either lapse into a state of hibernation with the consequent loss of the precious body of expertise that it now has, or it can, with judicious funding and tough decisions, continue to move forward in a vigorous manner. I hope you will agree with me and choose the latter course.

[The prepared statement of Dr. Coppi follows:]

PREPARED STATEMENT BY B. COPPI, PROFESSOR, MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Honorable Chairman and Members of the Committee: I wish to thank you for taking the time to listen to my views on the long and short term course of the U.S. program concerning the feasibility of experimental fusion reactors.

I—GENERAL CONSIDERATIONS ON IGNITION EXPERIMENTS

As you probably know, since the summer of 1975, I have been involved with proposing and designing experiments that have the purpose of proving that a plasma mixture of deuterium and tritium can ignite by fusion reactions. In fact, the Alcator experiments of MIT had shown, by that time, that existing technologies together with newly available information about the physics of magnetically confined plasmas could be used to initiate the construction of an ignition experiment. The proposed experiment was called Ignitor and, like Alcator, had compact dimensions, relatively high magnetic fields, and could operate with relatively high particle densities. Under these conditions we already knew that the expected confinement properties were good and that the plasma had a high degree of purity, a characteristic necessary for fusion burning regimes to be reached.

Since many of us have been waiting nearly thirteen years to see that an ignition experiment (and in particular one of the compact type as originally proposed) could finally be undertaken in the United States, I am of course anxious that we do not

have to wait another decade or more before an experiment of this kind is brought to significant operation.

I think the world needs to know whether a fusion reactor is feasible. Developing countries are particularly sensitive about this point. With all the constraints imposed by the environment, how can we satisfy the world's energy needs if the average per capita consumption were to become comparable to the levels of the most affluent countries? The affluent societies are sustained by the bountiful and inexpensive energy that is provided by fossil fuels. Shortages of these fuels, either actual or for environmental reasons, are not inconceivable in the foreseeable future.

Another need, that is no less important and impacts on other fields, is that for knowledge. This responds to our inner desire to see whether we can mimic, in the laboratory, the processes that keep stars burning and to understand the behavior of a form of matter (the plasma state) that pervades most of the universe.

The U.S. has an unique pool of talent and a well-established tradition of being able to carry out original and outstanding scientific enterprises within relatively short time spans. I think this ability is sorely needed in the worldwide effort to produce a working fusion burner.

In fact, it is my belief that the U.S. fusion program in spite of its past successes is now very close to entering a state of hibernation. This involves: (i) the questionable deuterium-tritium operation of the TFTR machine at the Princeton Plasma Physics Laboratory (PPPL) and the consequent rapid demise of this machine, resulting from the level of radioactivity induced in it; (ii) the slow construction and the late beginning of tritium operation of a large size and probably marginal (in terms of achieving full ignition conditions) CIT device; (iii) the looming doubt that the considerable quantities of tritium needed for a meaningful series of experiments by the presently proposed CIT and the problems associated with these experiments may not prove acceptable to the residents of the Princeton neighborhood; (iv) the prolonged idling of the outstanding experimental staff of the laboratory.

Meanwhile, most other groups in various academic and industrial institutions associated with fusion research continue on their way to extinction and emargination.

Recommendations and long-range value of compact experiments

The experimental test of ignition is an unusually well-justified scientific undertaking, that is long overdue. We currently lack any experimental knowledge of the physical conditions of plasmas that are effectively heated by the particles produced in a fusion reaction. In addition, I should say that the line of experiments represented by Alcator (that has been both cost effective and scientifically successful), Ignitor (that has received initial funding in Europe), and a compact ignition tokamak is very promising. It could, in fact, teach us how to burn more desirable fuels than deuterium-tritium—in particular deuterium-helium3 mixtures where the prevalent reaction does not produce neutrons.

Under the present schedule, experiments to approach ignition conditions may not begin until 1998 or later. I believe that this is much too long to wait. Therefore, I would like to see a greater sense of urgency in proceeding towards the construction of a compact ignition experiment, with both the technical and management effort being devoted to finding the most expeditious solutions that would allow meaningful experiments on deuterium-tritium reacting plasmas within the first half of the next decade.

Value of the TFTR/PPPL facility

Therefore, since it is evident that, in the absence of new funding, the proposed tritium experiments at the TFTR machine of Princeton Plasma Physics Laboratory will compete with a reasonably rapid construction effort for a compact ignition tokamak (a "CIT"), I would recommend that the tritium experiments be postponed indefinitely. This postponement would free up significant funds that could be used to increase the construction pace of a "CIT."

In addition to the possible conflict with a concurrent "CIT" project, there are other considerations which suggest that the tritium operation of TFTR not be performed. Among these are: (a) The possibility of achieving the so-called "breakeven", $Q=1$, condition in TFTR looks very uncertain at this time. It could be demonstrated more effectively in a compact machine. The value of achieving an improbable "breakeven" on TFTR is certainly not commensurate with the costs incurred by rendering the device radioactive and consequently losing a very valuable facility to further operation. We should remember that this machine has required a large investment in both time and resources and severe sacrifices from the rest of the interested scientific community; and (b) The number of discharges (shots) that could be produced with deuterium-tritium mixtures at full power in TFTR is very limited.

This would obviously hinder the execution of any substantive program of physics investigations.

If the tritium experimental phase of TFTR is not pursued, extended operation of other PPPL programs of crucial importance to the development of efficient fusion reactors would be assured. As an example, the permanent mothballing of the PBX-M machine could be avoided. A well-conceived series of investigations could deal, for instance, with (i) an increased understanding of the processes by which energy and particles are transported out of the plasma, (ii) the ability to approach new stability regimes (e.g., the so-called "second stability" region) where relatively high plasma pressures can be produced and maintained, (iii) new plasma heating methods, (iv) new plasma fuelling and exhaust methods, etc. This solution also avoids the danger of idling or, more likely, losing the best members of the Princeton scientific staff, which is an inevitable outcome given the large (and uncertain) gap between the shutdown of TFTR and the actual operation of the presently proposed CIT.

Therefore, it is essential that the funding freed by such a decision not be completely withdrawn from the PPPL program, and that part of it should be used to exploit the existing PPPL facilities to their full potential, with deuterium plasmas, in order to obtain vital physics information needed to improve the performance of future experiments.

II—ALTERNATIVE SCENARIO FOR THE U.S. FUSION PROGRAM

Since the letter of the Chairman, Honorable M. Lloyd, requests my opinion about possible alternatives to the presently chartered fusion program for the United States, I shall dwell on possible "radical" alternatives. Speaking bluntly, I think that, with a judicious addition of funding to the existing 5-year plan, this program could be converted from one where the U.S. effort hibernates until the year 2000 and probably beyond, to one that has current impetus and a vigorous future.

An "upbeat" option would first of all choose a site that is already approved for the operation of nuclear reactors, with containment buildings available and the immediate access to a node of an electrical grid that can serve as the power supply for the foreseeable generation of experiments. Several such sites, that all needed to ensure the nuclear aspects of the U.S. fusion program a healthy evolution, exist. This site will house an Advanced Ignition Experiment (A.I.X.), that will leave a high probability to read deuterium-tritium ignition and would also try to burn a deuterium-helium3 mixture, could be designed and planned.

Effective international cooperation

Considering the Fermilab of Batavia, Illinois, as a model, the new site should be operated as a truly national facility and become part of the worldwide network of (the few) sites that are being established in Europe, U.S.S.R., and Japan for the development of near term experimental fusion reactors. The resources, human and material, that the existing program allocates to ITER, for the sake of international cooperation, should be devoted to nearer term fusion burn experiments that are truly likely to be constructed, rather than to remain an exercise on paper. Given the extensive high level acquaintances I have in Europe and the Soviet Union, it is my firm opinion that experiments on such a site, if expeditious and well-conceived, will attract their participation. The history of the Alcator program at MIT is a proof of this. In fact, significant participation on a near term fusion burn experiment would lay the ground work for a more extended and large scale collaboration. Also, U.S. university and industrial groups should be encouraged and funded to contribute as active partners, again as in the case of elementary particle physics.

A proposal for the Princeton Plasma Physics Laboratory

Within this scenario, it is important that the Princeton Plasma Physics Laboratory remain a strong international center for plasma physics devoted to the theoretical and experimental study of the fundamental physical processes relevant to fusion reactors. The TFTR facility would be assured an adequate life that, scientifically speaking, would provide an appropriate return for the investment made in it. The present CIT effort would be converted into a CPT (Compact Princeton Torus) at substantially lower costs and faster schedule as it would no longer be burdened by the problem of being limited in its operation and accessibility by the levels of machine radioactivity associated with tritium operation. It would be sized to fit within the extisting test cell and the present already large, electrical power supply system, but would adopt innovative technological solutions allowing it to attain new and exciting physical regimes. Thus, the outstanding expertise in plasma physics that now exists at Princeton would be utilized at its best.

Summary

The United States is at a critical point on the road to demonstrating that fusion can be a real and possible energy source. It can either lapse into a state of hibernation with the consequent loss of the precious body of expertise that we now have or it can, with judicious funding and tough decisions, continue to move forward in a vigorous manner. I hope you will agree with me and choose the latter course.

DISCUSSION

Mrs. LLOYD. Thank you very much, Professor. I look with interest on your alternative scenario for the fusion program.

How do your goals differ from the goals of CIT?

Dr. COPPI. Well, as you have seen, I mean originally the CIT goals and mine coincided. It was supposed to be a small-scale and fast experiment. Now, because of budget and technical consideration I think that the constraint of this idea is making it a very, very long-range enterprise.

So, this is what I am proposing that we should choose a national site and do an experiment there at the fastest possible pace. In fact, I like the question of Mr. Morrison, when he asked what is the relationship of what we do to the inertial program. Well, whether or not they do burn tritium, they deal with the nuclear technology of fusion. We have yet to do that. And they think that this is long overdue. We have many sites where we could begin to do that, and this is what I have advocated that we do.

I don't think Princeton would lose at all in a solution of this type.

Mrs. LLOYD. Your thought about choosing a national fusion site is an interesting one. I look with interest on the prerequisites and it seemed to me the DOE lab in Oak Ridge might fit their requirements, I don't know about Hanford, but whether the site always fit with these requirements.

Dr. COPPI. I had forgotten that when I wrote this that you were coming from Tennessee.

Mrs. LLOYD. I don't want anyone in this room to think I'm being parochial on this issue, please don't, it's certainly not my intent. But, given the resources that we already have committed at Princeton in terms of power and the physical plant and people, do you really think it would be cost effective to move the ignition facility to another site?

Dr. COPPI. I think that of all the resources that exist at Princeton, first of all, the human one, in my opinion, is the most priceless one. And it is important to keep them working productively. I don't think that the investment made would be wasted by limiting the operation of TFTR, in the future experiment of deuterium only. We need to decide whether the program has a future and I do not see any waste of the Princeton resources, again, limiting their operation in deuterium.

Mrs. LLOYD. Your comments are certainly interesting regarding postponing tritium experiments. Already, it seems to me, that JET plans on doing tritium experiments in the early 1990's. Would you comment on your knowledge of the D–T experiments planned for JET?

Dr. COPPI. Well, JET is planning to do deuterium-tritium. I do not know whether that will be done for sure or not, but the plans

are there. But I don't know, frankly, even for JET, what the scientific significance is of that. Frankly, I would rather have preferred, as I indicated with my work, to do a truly D–T burning experiment where the heating of the plasma resulting from the fusion reaction product really is effective.

I objected basically to the European fusion program taking that direction because I think that those experiments, at least this is my personal point of view, they are not worth the cost.

Mrs. LLOYD. Do you think the objectives of the D–T experiments will be accomplished in the experiments on JET?

Dr. COPPI. No, you see, you need to prove that you can heat a plasma, that you can keep it lighted, with the heat that comes up from the fusion reaction product. The JET experiment has yet to achieve the confinement characteristics that are needed to prove that that heating could take place.

For example, one advantage that the machine has over JET is that it can work in a regime close to reactor core where plasmas are very clean. JET has not achieved that condition of cleanliness such as it would need in a true fusion burning plasma. So, I'm not so enthusiastic about the D–T experiment at JET, either.

Mrs. LLOYD. Very Good. Mr. Morrison?

Mr. MORRISON. Doctor, your ideas seem to be a little different than the direction that the program is taking. I just wondered, we seem to have some pretty good advisory groups that are working with the Department of Energy in this area, have you had a chance to visit with them or participate in any of their discussions?

Dr. COPPI. Definitely, I should tell you if I may be arrogant, that I've been associated with the program for a long time, and in fact, when the Tokamak came to this country in a big role, so I'm not an outsider.

If I look, for example, at the advice that was given for doing the TFTR tritium operation, I would say those are motivated by a rather negative view of TFTR. They felt that the machine has run its useful life, it has a number of shortcomings, and that this was the only thing the public could do. I think TFTR is like a magnificent telescope. We're just beginning to use it.

So, they know my different opinions. I can tell you that many people in the community share my opinion on that point of view.

Mr. MORRISON. We'd like to, on this Subcommittee, feel like we were moving in the right direction, so your views are refreshing and a little alarming, perhaps, because we have faith in the folks that are heading this program.

Dr. COPPI. But I am not—I don't think that my testimony goes against what the major direction of the program is. I would just like to see it speeded up. I would like to see an increase in the budgets, and I would like to see that we begin to do the nuclear engineering of the fusion program.

Mr. MORRISON. I share your concern. I look at that gap that comes from level funding between 1991 and 1996, and do have a very real concern with that time period, and would hope that we can do something about it.

You mentioned in your testimony, make reference that the looming doubt that the considerable quantities of tritium needed for the

CIT may not prove acceptable to the residents in the Princeton neighborhood.

Dr. COPPI. This is strictly my personal point of view. The present machine is a large machine in design, because it has to accomplish a number of goals at the same time. So, it will require a large amount of tritium. If you go around Princeton, you visit this multi-nuclear site, it is as evident to you as it is to me. I don't know. We never know whether—are you sure we will really be able to do those experiments? Remember the present CIT, if it would ignite, it is half a gigawatt, 500 megawatts of fusion power. If not, it is a reactor.

Mr. MORRISON. Well, that is a concern and I just wanted to sound you out on it. Obviously, you have raised some questions about funding and even direction of the program, and those views are appreciated.

Dr. COPPI. Mr. Morrison, I would like to say something. I work at a university and I don't know if you appreciate the level of misery that we have been living through in these years. And now, I don't know how we can survive with that five-year plan. We don't have that money to support the graduate students that come to us, even when the children of my friends applied to MIT, I could not take them because I had no money. The situation at the university, as probably Dr. Fleischmann I'm sure will indicate to you, is very dramatic.

So, this flat budget, over five years in this course, I think is devastating for us.

Mr. MORRISON. We heard some excellent testimony yesterday pointing that out and I think this Subcommittee is very concerned in that area for the leadership for tomorrow that we're losing by not attracting those students today.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Morrison. Thank you, Professor Coppi, for your very enlightening testimony. We appreciate you being with us as well.

Our last witness today is Dr. Hans Fleischmann, from Cornell University, who is President of the University Fusion Association. We do welcome you, Dr. Fleischmann. Thank you for being patient and staying with us as our last witness.

Your written statement will be made a part of the record, and you may summarize as you wish.

STATEMENT OF DR. HANS FLEISCHMANN, PRESIDENT, UNIVERSITY FUSION ASSOCIATION, AND PROFESSOR AND ASSOCIATE DIRECTOR, APPLIED AND ENGINEERING PHYSICS, CORNELL UNIVERSITY, ITHACA, NY

Dr. FLEISCHMANN. Madam Chairman, Members of the Committee, I should thank you for being invited and not the other way around, and also to come back again for the University Fusion Association, who was before you before. I think what I will do is maybe really to summarize or maybe highlight or re-highlight whatever I wrote, but keeping the same sequence that I did so far.

I think maybe I should first say we are an association with rather widespread membership. I think we represent in that sense

the universities quite well. In the preparation for this testimony I have talked with a sizeable number of people in order to be really a representative of that body in a decent fashion.

I guess regarding the Administration proposal, I'm on page 2, there are basically two parts to it. On one hand, the ignited thermonuclear plasma in a tokamak type machine, as you heard—in that regard I would say the universities, and if I talk in that fashion now, I think will support and understand the basic importance of a drive for ignition with regard to importance physics-wise and also regarding maybe the visibility of the program to get on and to see what happens in that area.

I think that is very important first step toward fusion. We have to do it at some place. There are a number of questions that are looming and I think they need to be investigated. As I see it, and as I think many of us see it, I think there will be one machine of this type. One does not have the money to have more machines, as we had in the past. It will be expensive, most likely, if we don't find really a much cheaper way, which I have no idea about, but most likely it will be expensive but if properly designed, funded, and executed, I think its expenses and we think its expenses are justified by the progress it would permit—probably highly justified.

I guess on the other hand we do have in that regard one concern, and that regards the start of that project under the present, very serious and very restrictive budget conditions. In particular, if we take the five-year out projection as a standard, the concern goes mainly toward I guess what also Dr. Furth called the base program. I guess I should define it at the moment essentially as a program part that does not provide the most visible front-line numbers that can make headlines in newspapers and so on. But the program is very important in the end, as I see also Dr. Furth pointed out, for the progress of the program, for the possibility to achieve those highlights. That includes, if we do it budget-wise, basically most of the program outside the CIT planning and CIT TFTR, maybe some support machines, it is some grey areas which one could possibly count both ways or that probably would still count also—generally doubled to that, to the frontline issue, I think it is probably better to call that part of the program with that name.

The rest is essentially baseline. Now that consists of a number of programs, as already Dr. Furth pointed out, on one hand a number of direct supporting experiments to improve Tokamaks, the Princeton Tokamak PBX-M, a number of other programs. Basically they test various improvements in that case on Tokamaks. There are various other schemes, various other points. They include materials testing, materials improvement research for future machines not presently for, say, CIT. It includes also and I really think very important alternate concepts, alternate concepts being at the moment RFP, stellarators and maybe some were more emphasized than had been done so far, I think, compact toroids.

Those programs are basically programs that should in the long run assure that we will have the best product available in the final machine that will produce and go hopefully to the market.

It is this part of the program, and I probably didn't necessarily hit all of those parts. If I overlooked one, please forgive me. It is

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this part that I think people are concerned about. The funding of our program so far over the last period as all of the programs has gone down, has suffered. There are quite serious problems. A number of programs have been shut off, and not necessarily technical but for financial reasons, a number of things maybe I can go in later.

This base program, if the further cuts come in particular as part of a just CIT emphasis, then I think I and many people really are afraid, and if you want to question later, it might be worthwhile, but then people are afraid that more—that this program may really deteriorate as also Dr. Coppi indicated.

It is our feeling that that part of the program really is important, has been important in the past and still is important in the future. I have given here a few examples, for the past, I may read off the high-field Tokamak concept is an outstanding example where the idea initially hatched and tested by Professor Coppi and his co-workers at MIT and subsequently developed in successively larger experiments, now is at the base of the planned CIT device. It is very clear it came out of at that time base program, and was clearly not designed at that time for frontline experiments.

Similarly, current drive by microwaves—now these are methods for permitting long time steady-state discharges in Tokamaks, as will need them in reactors, have first been hatched in university size groups, both here and abroad, and then brought to the stage where this idea now is being considered for the large international ITER program.

Another example is the stellerator concept which, after having been abandoned at, I have to say, at PPPL and later having been shown in Europe to have very good confinement prospects, was reintroduced to the U.S. really by a university group in Wisconsin and is now being investigated in the larger ATF experiment in Oak Ridge, as I am sure you are aware.

In all these cases, as in many others, the new idea had to be tested, modified, and proved in a hierarchical progression on increasingly large and increasingly less flexible experiments—problem if you go to larger experiments, if you do something wrong, you have much more trouble than if you do it in smaller ones—until it could be included with sufficient confidence in frontline experiments aimed at driving plasma parameters into a new regime.

Similarly, most of our best diagnostic methods first were tested and developed on small, flexible experiments before their inclusion in large experiments. Major experimental failures and misconclusions in fusion research had their origin in an insufficient recognition of this relationship. Sometimes we make too big steps and we can't help it. Yes, it is a judgement problem or question always.

As a small example, I guess I was writing a parochial point to our own experience, where it was, I think a very small experiment could show that at the conclusion from a larger experiment that it wouldn't work, that this concept wouldn't work clearly was out of line and was wrong, that one could get it to work, with money, very small money compared with what the large experiment spent.

Let me go on, then, next step. I think we feel that it is important also still including this base program also will be crucially needed in the future. In view of the envisioned final economic use of fusion

reactors, it is not sufficient just to produce energy but this energy has to be generated in the technologically and let me add also environmentally and economically best manner.

We have to provide the best product in the end. Say it also has to be safe. Again, a short and badly incomplete list of a few examples of future needs should suffice. Tokamaks I would maintain still require—and I am sure also that Furth will agree—will require various improvements. One still would like to get higher beta. One would like to get better confinement to make it more economic, make it more saleable.

Several alternate concepts including field-reversed pinches, compact torroids and stellerators have the inherent potential to provide more economic final reactors and/or a better understanding of physics issues in Tokamaks. I think it is also important to understand Tokamaks, some of the physics in particular confinement, still better in Tokamaks.

At present, those alternate concepts are investigated on a level comparable to that of Tokamaks 10 to—and I maybe even would say 20 years ago—progress is seriously slowed by limitations, by financial limitations which permit only very few simultaneously existing experiments. That I feel is a very serious problem. In Tokamaks, in the very successful progress of Tokamaks, there have always been four or five, maybe ten machines of similar variations, of Tokamaks in the world. It was always possible to point to one that just produced. Every experiment takes a while until it really produces. Some may not produce at all and that does not mean—and some Tokamaks did not produce, it did not mean that the Tokamak concept is wrong. If we have only one or two machines in alternate concepts, then it normally is taken as no, just that the concept is bad.

I think that is a serious problem if we really strive for the best fusion reactor, we should remind ourselves—let me read—how problematic the premature narrowing of reactor options proved in the fission reactor program. I think we had some problems there. Clearly, these alternate concepts need to be investigated and we think harder, and if physics-wise successful, brought closer to a level where they can compete with Tokamaks. Similarly, improvements and optimizations of reactor materials and technologies will be decisive in economic, technologic and environmental respects. Further, basic physics investigations, in particular on turbulent plasma diffusion, may lead us to a much better pre-determination of plasma behavior in new machines than our present. Still, I would say, rather coarse empirical scaling laws permit, resulting potentially in very sizeable savings and avoidance of potentially costly mistakes—if we get to large machines we have always and one has to expand present knowledge. One does not, if I have one machine exactly that problem, I do not want to find out that this machine doesn't quite work and then be told look, maybe that concept, maybe all fusion is wrong.

As mentioned, these are only a few examples for the long-term health of the overall program, a healthy base programs with its influx and sifting of new ideas we feel is still absolutely essential.

Last, not least, I think I will again thank you for letting me speak here to you and obviously I am ready for questions.

[The prepared statement of Dr. Fleischmann follows:]

STATEMENT ON THE MAGNETIC FUSION BUDGET

PRESENTED TO THE

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

OF THE

HOUSE COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY

FOR THE

UNIVERSITY FUSION ASSOCIATION

March 30, 1988

Hans H. Fleischmann, President, UFA, and
Professor and Associate Director
Applied and Engineering Physics
Cornell University, Ithaca, New York 14853

Madam Chairman, Members of the Committee:

I am coming before you as a representative of the University Fusion Association, which you may remember from similar earlier occasions. With a present membership of about 140 active researchers in 35 universities, our Association contains a representative cross section of the full fusion community in universities. During the preparation for this testimony, I sought and received the views of a sizable representative sample of senior university researchers, which collectively represents a community that is technically involved in essentially all aspects of, and highly dedicated to, the goal of making thermonuclear fusion a useful power source for us and our children. I will present to you a carefully balanced aggregate of the vast majority of this body. I will be brief and try to provide any further details you may wish either here orally or in subsequent written communication.

The effort proposed by the Administration essentially contains two major parts. One of these concerns the near term drive toward the achievement of an ignited thermonuclear plasma in a tokomak-type machine. Physically, this includes the already existing Tokamak Fusion Test Reactor (TFTR) device together with a number of existing directly related experimental, theoretical and technological efforts; in addition, it also includes a specific proposed new and rather large device, the Compact Ignition Tokamak (CIT). On this part, the university community recognizes as important the basic goal of obtaining and investigating the first controlled ignited plasma on earth; such ignition clearly is an important necessary step in our quest towards fusion energy. First studies of the ignited plasma state still can be performed using existing machines. More detailed studies will require a new device which will be quite expensive, but, if properly designed, funded and executed, its expense is justified by the progress it permits.

However, the university community shares a serious concern about starting such a device under the present highly restrictive budget climate, in particular in connection with the present 5-year flat-effort budget projection. Some of this concern applies to the device itself where the budget limitations will seriously strain the success chances of the machine. Moreover, a most serious concern exists with regard to the remaining second part of the Administration proposal, i.e., the base program of the OFE fusion effort. The health of this program part has been severely impaired by the budgetary needs and restrictions of the last years. Any additional constraints and effort reductions brought on by the planned device construction, I expect, will lead to serious problems including a possible paralysis and widespread defection of good people. Obviously, these problems would be further exacerbated if future budget appropriations prove less than

level-effort or if the cost of the CIT escalates; the proposed overall budget of 360M$ for 1989 already constitutes an overly tight lower limit.

This base program encompasses a wide range of experimental and theoretical efforts including many directed at developing and testing new ideas and concepts--both for improving existing confinement concepts and developing alternate, potentially better ones--efforts for developing advanced generic technology and materials, general system studies, basic goal-oriented physics and engineering studies, the training of new people and others. Universities carry a sizable part of this effort, but such studies also are extensively pursued at various industrial and national laboratories.

This base program, although not directly related to the frontline machines, proved very important for the fusion program in the past, and it will still be of crucial importance in future for the development of a realistic, practical fusion reactor.

Examples of past achievements abound, but let me concentrate on a few university contributions in the concept areas--I hope without prejudicing or reducing other equally important areas and efforts: The high-field-tokamak concept is an outstanding example where the idea, initially hatched and tested by Prof. Coppi and his coworkers at MIT, and subsequently developed in successively larger experiments, now is at the base of the planned CIT device. Similarly, "current drive by microwaves," that is, methods for permitting long-time steady-state discharges in tokamaks, have first been hatched in university-size groups both here and abroad, and then brought to the stage where this idea now is being considered for the large international ITER project. Another example is the stellerator concept which, after having been abandoned by the PPPL and later having been shown in Europe to have very good confinement prospects, was reintroduced to the US by a university group

in Wisconsin and is now being investigated in a larger ATF experiment in the Oak Ridge National Laboratory. In all these cases, as in many others, the new idea had to be tested, modified and proved in a hierarchical progression on increasingly larger and increasingly less flexible experiments until it could be included in a "frontline" experiment aimed at driving plasma parameters into a new regime. Similarly, most of our best present diagnostic methods first were tested and developed on small, flexible experiments before their inclusion in large experiments. Major experimental failures and misconclusions in fusion research had their origins in an insufficient recognition of this overall relationship. As a small example, I might be permitted to be parochial and point to our own experiments where in the early 1970s failure of the large A tron experiment at LLNL was taken as a sign of non-attainability of the goal of the experiment. Starting much later, but using a different method in a very small flexible experiment, we obtained the desired results.

As mentioned, such contributions of the base program also will be crucially needed in the future. In view of the envisioned final economic use of fusion reactors it is not sufficient just to produce energy but this energy has to be generated in the technologically and economically best manner. Sizable improvements still are needed on all fronts. Again, a short and badly incomplete list of a few examples should suffice. Tokamaks still require various improvements, for example, higher beta and better confinement. Several "alternate" confinement concepts, including reversed-field pinches, compact toroids, and stellarators, have the inherent potential to provide more economic final reactors and/or a better understanding of physics issues in tokamaks. At present, they are investigated on a level comparable to that of tokamaks 10-15 years ago, and progress is slowed by their financial

limitations which permits only very few simultaneously existing experiments. (In contrast, there always were several first-rate tokamaks in the world during the basic development of that concept.) We should be reminded how problematic the premature narrowing of the reactor options proved in the fission reactor program. Clearly, these concepts need to be investigated harder and, if physics-wise successful, brought closer to a level where they can compete with tokamaks. Similarly, improvements and optimization of reactor materials and technologies will be decisive in economic-technological respects. Further, basic physics investigations, in particular on turbulent plasma diffusion, may lead us to a much better predetermination of plasma behavior in new machines than our present rather coarse empirical scaling laws permit, resulting in very sizable savings and avoidance of costly mistakes. Finally, a healthy theory program is needed in cooperation with these efforts for optimization and full fruition of these efforts.

As mentioned, these are only a few examples. For the long term health of the overall program, a healthy base program with its influx and sifting of new ideas still is asbolutely essential.

Last, but not least, we would like to thank you for your willingness to again hear our opinion and also for the support your Committee has shown to us and the entire fusion community through the years.

DISCUSSION

Mrs. LLOYD. Thank you, Professor Fleischmann, for your testimony. I would like to follow up on your concern over a balanced program in fusion and the ability to still fund the CIT.

How much of the proposed 1989 budget is part of what you and Dr. Furth called the base program? And how much is university research?

Dr. FLEISCHMANN. Let me say, I guess I didn't point that out in my oral testimony. The base program does not only include the universities. Universities are only a relatively small percentage—not that small—depending on how we define it—but not such a big percentage of the program.

What I was talking about was really a wider program was the national labs and so on. Universities are heavily in the base program, but it is wider than that.

Your question of the universities, is nowadays, if and including MIT, which takes about half, is sometimes also considered the national lab by some people, its $50 million, $25 million for the rest of the universities.

The base program itself we first went through, is probably about 50–60 percent, depending on exactly how one defines it.

Mrs. LLOYD. Now, 50 to 60 percent is part of the base program, and that includes universities?

Dr. FLEISCHMANN. It does include universities.

Mrs. LLOYD. I think it's pretty hard to always maintain a balance between the applied thrust and, of course, supporting research. What do you think would be a good balance of funding between these two parts of the fusion program?

Dr. FLEISCHMANN. Probably—I haven't really thought much about that in detail. To some extent I would say in a normal program if I had only directed the front line efforts and so on, then as a backup, maybe 50 percent may be sufficient.

Mrs. LLOYD. I know that——

Dr. FLEISCHMANN. But we need ultimate concepts, also.

Mrs. LLOYD. I know that with your background there must be other government applied research programs that had a reasonable balance, and that's why I was interested in providing your insight.

Dr. FLEISCHMANN. I would think that—the past, I see it from the ultimate concepts, I think show a somewhat shorter than the main program. On the other hand, I have to say I work in that area, and sometimes the grass is greener on the other side. But, I would still say that if I balance everything, probably was not so bad a balance. I would say it was a bit shorter on the base program site but it was not grandly offered. Our concern, I guess, mainly applies to the future.

Mrs. LLOYD. Future of the fusion program?

Dr. FLEISCHMANN. If one would have constant budget projections, and then, I think, even to go out and build a large machine. And in particular, a machine which looks like it cannot be stretched out even further, in case it gets more expensive. I think that is very dangerous for the base program. That is what, I guess, is the concern of the people I have talked to and, as I said, I have talked to a sizable number of people that way.

There are some which think no, it's fine, and so don't let me mislead you. But the majority, and vast majority, really is——

Mrs. LLOYD. So, it depends on the program where you think the balance should be?

Dr. FLEISCHMANN. At the moment the balance I think is not unreasonable, but I think one should not shift it to what's more front line machines.

Mrs. LLOYD. Turning to another area of concern of this Subcommittee, certainly is the health of the university program as it relates to the production of graduates in the fusion programs. You know, this is a major concern of this Committee. Do you think we have enough graduates to supply the current need of the fusion program?

Dr. FLEISCHMANN. I think, as far as current needs, we have sufficient, but I'm not sure if it goes on that way, if we really will have. It starts, as I said, I think the last five years, as they were hard on everybody, also really were hard on the universities. And we support fully Professor Coppi there.

The danger is now, I have to say, the universities, let me again not produce a wrong impression, universities were not badly treated by DOE. Universities were about, percentage wise, some were better treated. Let me make that clear. But still, it is very problematic, particularly in small universities, the number of universities went from 41 to 32. It did hit mostly small ones. The bigger ones were still okay.

Mrs. LLOYD. Is there a demand for fusion, for graduates of fusion research——

Dr. FLEISCHMANN. From what I know, they certainly all could still get jobs in the program. The problem nowadays is that the outside picture for students is no longer attractive. That means that the number of good students coming in is dwindling quite sizably, so it's the picture outside.

It has two reasons on one end, maybe the dream is not as visible for those students, but also the fact that the program has gotten cuts, we have bad problems in funding. As an example, I have to not change something on my machine which normally cost them, realistically and on the market, is probably near $.5 million, maybe $300,000, maybe $500,000, I think nearer to $.5 million, if I would directly buy it. I have to do that with about $70,000, which I scratched together.

It is very difficult. If that holds on, we have old equipment, we don't get new equipment. That all acts as a drawback to students. It gives the faculty, also, the problem that's as enthusiastic—it's harder to project. We have also the responsibility, in particular now, if we look at the five year projection, we have the responsibility, if we accept students, and I think we have a responsibility towards them as well as toward the university to make sure they get through; they don't get in between, suddenly stopped.

DOE, so far, has been good at that. I'm not putting this in as a complaint, and also not as a complaint for overall funding. I think universities do recognize the your problems and the country's problems. But, if it goes further beyond that, then I am afraid that, at someplace, can become a disconnect. I'm really afraid of that.

Mrs. LLOYD. Thank you. Mr. Morrison?

Mr. MORRISON. Dr. Fleischmann, you might be interested to note that from yesterday's testimony we heard that a lot of the graduates in physics were going on to medicine.

Dr. FLEISCHMANN. Undergraduates. Ph.D. graduates?

Mr. MORRISON. Probably undergraduates.

Dr. FLEISCHMANN. Undergraduates, yes. In our case, in engineering physics, I guess I have not tested really overall——

Mr. MORRISON. Presumably attracted by the chance to make more money out in the——

Dr. FLEISCHMANN. Yes, and also, to have more activity. I think the problem, or the point let's say, is in the superconductors or in electric engineering computers, great activity, not contracting.

Mr. MORRISON. Your testimony, you seem to express some concern about building a big machine during a time of tight budgets, and wanted us to make sure that we didn't lose sight of the contribution that smaller, more flexible machines have made in the area of fusion.

Dr. FLEISCHMANN. I guess, even more so than often, they are still needed in the future.

Mr. MORRISON. I understand your concern and I think that we're going to have to do some—make some special effort as we head into a new administration to have them understand where we really are in the fusion arena so that we don't have to consistently live within that sort of a tight, almost impossible, scenario.

We've had some interesting testimony this afternoon and you've listened to both Dr. Decker and Dr. Furth, and the slightly different approach from Professor Coppi, and I'm wondering if you'd like to take sides between the different angles we've had on the subject of——

Dr. FLEISCHMANN. I don't think I should do that as a representative of the University Fusion Association. One of the problems I think we do have on that is always that we should not just take sides between our membership.

Mr. MORRISON. Is it fair to say, and I think this was common throughout all of your testimonies, all of you that have been here today, that more money would save us from having to make some difficult decisions that perhaps divide us as we move on toward to the goal of fusion energy?

Dr. FLEISCHMANN. Yes. I've made in my written testimony a statement which I should, I guess, repeat. That I think that this present $360 million certainly is, I guess I said, an overly tight projection for the next year in that if one then stays flat in his CIT, that is certainly too low. That will be in my statement.

Mr. MORRISON. Thank you very much, Madam Chairman.

Mrs. LLOYD. Thank you much, Mr. Morrison. Dr. Fleischmann, thank you for your testimony. All of our testimony has been excellent today. The witnesses did an admirable job of preparing their statements. We thank all of you for being here, not only our witnesses, but those of you that are in the audience for your concern of our fusion program.

And I'd like to thank our staff today. They've done an excellent job. Dr. Oran has recently joined the Committee, we're proud of your contribution today; and certainly, Kitty, you've done an excellent job as well.

If there are no further comments, then the Subcommittee stands adjourned.

[Whereupon, at 3:05 p.m., the Subcommittee recessed, to reconvene at the call of the Chair.]

APPENDIX 1

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE. MASSACHUSETTS 02139

April 7, 1983

Honorable Marilyn Lloyd
Chairman, Subcommittee on Energy
Research and Development, B-374
Rayburn House Office Building
Washington, DC 20515

Dear Honorable Lloyd:

I wish to thank you again for the cordial reception I found at the hearings and take the liberty to write you some of my thoughts that followed the exchange of opinions we had.

First of all, since I have enjoyed an excellent personal relationship over the years with Dr. J. Decker who heads DOE's research program and with Dr. J. Clarke, I think I understand their positions and I sincerely sympathize with them. In addition:

i) Both Dr. J. Clarke and Dr. J. Decker have recognized the importance of and the need for an ignition experiment. Therefore, we all share a strong desire to bring this to reality.

ii) Also, all of us recognize that the most cost effective use of the fusion program's resources requires that the high quality PPPL facility be used in some post-TFTR experimental effort. It would be extremely wasteful to allow this program to lay fallow for a very long period of time; it is important that long and unacceptable gaps be avoided.

iii) I recognize that Drs. Clarke and Decker are doing their very best to make it possible, within an exceedingly tight budget, to satisfy both needs i) and ii) by a CIT device at Princeton.

iv) Princeton has attracted a talented and diversified staff and, therefore, it needs a flexible scientific device to explore a wide range of plasma physics phenomena. This has influenced the CIT design. In these circumstances, I think that, as indicated in my testimony, the capabilities of Princeton's PPL would be far better utilized by a hydrogen and deuterium experiment (the CPT) that can do more physics than the present CIT, would cost less than one-half of it and could be completed in significantly less time. Also, Dr. H. Furth indicated in his testimony that he was more interested in developing smart tools than using "brute force" to achieve a high level of performance in a tokamak. A machine that is dedicated to deuterium operation will be definitely superior to the proposed CIT in accomplishing this task.

v) By avoiding the questionable D-T experiments in TFTR substantial D-T preparation and operation dollars are saved, TFTR can run longer and concentrate on "smart" physics, and the TFTR test cell is saved from activation, which permits its reuse for CPT.

vi) An ignition experiment (A.I.X.) carried out at a nuclear compatible site with an easy access to a node of a strong electrical grid could be pursued immediately and expeditiously with current CIT funding. In fact, I believe that it could be accomplished with less cost by a more austere approach focused on ignition and burn only.

vii) Establishing a National Fusion Site is necessary not only to make this possible but also to ensure a healthy and rapid development of the nuclear aspect of fusion research. Taking this step now would also put the U.S. in a strong position to have an international advanced fusion burner sited in the United States. This could, in fact, give a concrete and exciting objective to the proposed ITER studies.

As indicated at the Hearings, I believe that my suggestions could be implemented with only a small increase in the total MFE budget. To illustrate this I have attached a rough and tentative MFE budget that would require increasing the presently projected MFE yearly allocation in the range 8 to 11%. I have also listed all the major advantages of this budget.

I remain available to provide any further information that you or your collaborators may deem necessary.

Yours truly,

Bruno Coppi

Bruno Coppi
Professor

cc: Dr. J.F. Clarke, U.S.DOE
Dr. J.F. Decker, U.S.DOE
Dr. D.B. Nelson, U.S.DOE

TABLE I

PROGRESSIVE U.S. PROGRAM

(Large Scale Experiments and Base Program)

FY	88	89	90	91	92	93
AIX (M$)	16 (CIT)	53	79	93	95	102
TFTR (M$)	72	48	45	40	30	-
CPT (M$)	-	20	25	30	40	55
Base Program Addition (M$)	-	10	20	20	20	30
TOTAL	88	131	169	183	190	187
INCREMENT ABOVE DOE PLAN FOR LARGE SCALE EXPERIMENTS (TABLE II)	0	30	40	40	45	44
PRESENTLY PROJECTED TOTAL MFE BUDGET	335	360	375	392	409	426
PROPOSED PROGRESSIVE TOTAL MFE BUDGET	335	390	415	432	454	470
% INCREASE OVER PROJECTED BUDGET	0	8.3%	10.7%	10.2%	11.0%	10.4%
CUMULATIVE AIX	16 (CIT)	69	148	241	336	438
CUMULATIVE CPT	-	20	45	75	115	170

CAPTION OF TABLE I: PROGRESSIVE U.S. PROGRAM

Basis:

- The new Compact Princeton Torus (C.P.T.) replaces the TFTR device at Princeton's PPL.
- TFTR is assured its proper utilization by avoiding deuterium-tritium operation.
- The Advanced Ignition Experiment (A.I.X.) is constructed at the National Fusion Site.
- A.I.X. Startup is foreseen for 1993.
- A.I.X. deuterium-tritium experiments start in 1995/96.
- An addition is included to avoid irreparable decline of the base program.

Advantages:

- Locates A.I.X. at a long term nuclear site providing a future to the nuclear development of the U.S. fusion program.
- Provides an outstanding, "smart" physics facility for Princeton's PPL.
- Permits the reuse of the TFTR test cell, eliminates the probable environmental problems that a large deuterium-tritium burning experiment can cause in the Princeton area.
- Provides the base program a means to develop electron cyclotron heating for ignition grade plasmas. This being perceived as the best heating system yet available.
- Focuses the U.S. burning plasma program on accomplishing the A.I.X. experimental effort and strengthens the U.S. ability to attract international collaboration.
- Strengthens the university program and the cost effective development and utilization of experiments at U.S. universities and laboratories.
- Enables TFTR to run longer and concentrate on "smart" physics while also enabling the new CPT device to be fabricated and installed in place of TFTR, thereby minimizing the activity gap at PPPL.

TABLE II

PRESENT DOE PLAN

Large Scale Experiments

FY	88	89	90	91	92	93
CIT	16	27	46	64	95	101
TFTR	72	74	83	79	45	42
TOTAL (M$)	88	101	129	143	140	143
Cumulative for CIT	16	43	89	153	248	349

BASIS:
- Carry out tritium experiments by the TFTR machine in 1991.
- Startup the CIT machine in 1996.
- Carry out deuterium-tritium burn experiments in CIT during 1998/99.
- Decline in basic plasma physics programs; no small initiatives.

Fusion Power Associates

Stephen O. Dean, President

April 8, 1988

The Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy
Research and Development
Committee on Science, Space
and Technology
Suite 2321 Rayburn House Office Bldg.
Washington, D. C. 20515

Dear Marilyn:

Thank you for your letter of March 30. As it turned out, I would have been able to participate in your hearing on fusion energy on March 30 if I had known that the hearing date had been changed from its original date of March 24. Such is life!

I believe that my views on the question you ask are largely covered in my original testimony which I submitted for the record. As I indicated, it is going to be very difficult to do all that DOE would like to do if the budget does not grow some in the next few years.

With respect to the TFTR, my view is that the proposed deuterium-tritium burn should be pursued if DD breakeven is achieved this year; if not, I do not believe it is worthwhile to pursue DT in TFTR. With respect to the CIT, since its schedule has been stretched out in any case, I would limit it to "Title I" design only for the next 2 years with the aim of finding a design that is smaller and less expensive than the current design point. CIT has gotten uncomfortably large of late. With respect to ITER, my view is that we need to participate in the design effort but that we should have a parallel U.S. design (as the Europeans and Japanese do) of an engineering reactor that is smaller and less expensive than ITER seems destined to be. In that way the U.S. may be able to afford a national test reactor in the 1990's.

I hope these comments are useful to you.

Sincerely,

Stephen O. Dean

cc: Dr. William Oran

2 Professional Drive, Suite 248 • Gaithersburg, Maryland 20879 • (301) 258-0545

FUSION POWER ASSOCIATES
2 PROFESSIONAL DRIVE, SUITE 248 • GAITHERSBURG, MARYLAND 20879
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STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY
MARCH 24, 1988

The effort to develop a fusion energy source has two important and equally essential parts. The first is, in the near-term, to demonstrate fusion plasma conditions of density, temperature and confinement time close to what is required in a fusion reactor. This effort includes developing the necessary technology and scientific understanding for those near-term achievements. The second important and essential element of fusion development consists of those programs required to ensure that fusion will be useful for some practical purpose in the long-term. These programs include concepts and concept improvements whose environmental, safety and economic features will make fusion a winner in comparison with competing technologies. Such efforts include the necessary systems studies and analysis, materials and technology development, new concept experiments, and fundamental physics and engineering research both at national laboratories, industries and universities.

Maintaining the necessary balance between these essential near-term and long-term aspects of fusion development has become especially difficult for both the magnetic and inertial fusion programs because of the decline of their budgets in recent years and the flat funding projections of the next several years.

In magnetic fusion, the near-term program consists primarily of the TFTR and CIT tokamak programs at Princeton, other large tokamak experiments, notably the DIII-D at GA Technologies and the Alcator program at MIT, and supporting theory and technology efforts. I would also include in this category the ITER design effort, although its payoff might well be categorized as "mid-term." These programs are clearly essential to fusion progress and deserve a certain degree of priority in allocating funds.

It is unfortunate that the funding projected to be available for the CIT project does not permit it to be built expeditiously. As I warned in my testimony last year, "Unless substantial increases are provided in FY 1989, I would anticipate schedule slippages and project cost increases." The budget submitted does not provide the increases required for expeditious completion either for CIT or for several other projects, notably the CPRF at Los Alamos and the Field Reversed experiment at Spectra Technology.

Of more concern than the FY 1989 submission however, is the out-year projections. The OMB and DOE appear to have agreed on a 5-year projected flat budget profile that, if implemented, not only results in delays and cost increases for CIT but also spells disaster for many, if not most, of the programs required for the success of fusion in the long-term.

Systems studies, necessary for guiding the magnetic fusion program toward practical products, are projected to decline to a paltry $2 million per year. These studies include the investigation of concepts and fuel cycles that would impact the environmental, safety and economic characteristics of fusion devices. Also, a recent subpanel of the Magnetic Fusion Advisory Committee recently warned in its interim report that the program currently in place and projected would not result in sufficient data by the year 2005 to permit an assessment of the environmental, safety and economic aspects of fusion to be made nor to allow a fusion demonstration reactor to be built using low-activation materials.

It is not unreasonable to ask the long-term payoff portions of the fusion program to tighten their belts a little in FY 1989 to help launch a major new near-term project like CIT. It is unreasonable, however, for the executive and legislative branches to expect to build a $400 million new facility without providing new money for a substantial fraction of its cost. A new project of this magnitude requires some real budget growth. This problem is not too acute in FY 1989 but will become very acute in later years if the 5-year projections of DOE and OMB are adhered to.

A similar problem has presented itself in the case of the inertial fusion program. This program has had good experimental success in the last few years and is now ready to begin design leading to construction of a "Laboratory Microfusion Facility." Unless real growth in funding is forthcoming, design and near-term R&D for such a facility will restrict our ability to fund those aspects of the inertial fusion program which have long-term civilian payoff. These programs include fusion reactor conceptual design studies and the development of laser and particle beam technologies that are capable of the high efficiency and repetitive pulsing necessary for a commercial power system.

Of immediate concern to this Committee should be the failure of DOE to request funds for the $27 million ILSE heavy ion fusion accelerator at LBL. Heavy ion fusion accelerator development has been a part of DOE's Basic Energy Sciences programs. As a consequence of DOE deciding not to fund the ILSE accelerator, they are also seriously considering abandoning heavy ion fusion accelerator development altogether. I urge this Committee to authorize the ILSE project and to direct the DOE BES office to continue this important program.

Fusion research and development is important to our nation's future. The fusion community appreciates the support it has received from this Committee over the years.

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Statement of the

Institute of Electrical and Electronics Engineers, Inc.

on

Fusion Energy Research

to the

Subcommittee on Energy Research and Development

House Science, Space and Technology Committee

April 14, 1988

Statement of the

Institute of Electrical and Electronics Engineers, Inc.

on

Fusion Energy Research

to the

Subcommittee on Energy Research and Development
House Science, Space and Technology Committee

April 14, 1988

This statement is made on behalf of the Energy Committee of the Institute of Electrical and Electronics Engineers (IEEE), the world's largest engineering society with over two hundred thousand members living and working in the United States. Approximately 80% of the membership is employed in industry, 10% in academic institutions, and 10% in various government laboratories and agencies. The Energy Committee, with representatives from 32 different technical societies within the Institute, is charged to develop the Institute's energy policy which is then reviewed and promulgated to the public.

We of the Energy Committee of IEEE continue to believe that fusion possesses a very high potential as a long-term energy source which has the advantage of a practically inexhaustible fuel supply which is accessible to all nations.

We again commend this Subcommittee on its strong support for the fusion effort over the years. This subcommittee has been instrumental in supporting the "burning core experiment," which was recommended by the Department of Energy's Magnetic Fusion Advisory Committee over the past years: the Compact Ignition Tokamak (CIT) is included in the FY88 budget with $15.5M ($8 of which is PACE-Plant and Capital Equipment.) This initiative will provide the data for the assessment of the effects of strong plasma self-heating and of a significant population of fusion products, one of the few remaining physics questions to be answered in currently planned experiments as input to the design of a prototypical fusion reactor operation.

In the past year, the fusion program has made considerable progress. On the path to the break-even deuterium-tritium (DT) fusion experiments in 1990, the Tokamak Fusion Test Reactor has reached a world-record plasma ion temperature of 300 million degrees Centrigrade. In addition, 27 megawatts of neutral beam power can now be injected into the plasma. An additional 7 megawatts of heating power from the radio frequency heating system will significantly enhance the machine's performance. The DT break-even experiments are planned in FY90-91.

Panel XVII of the Magnetic Fusion Advisory Committee emphatically endorsed carrying out the proposed TFTR experimental program, recognizing the importance of the mastery of the physics of alpha particle heating and the technical benefits of preparing and operating a tritium-compatible and radiation-hardened tokamak for the fusion program.

The progression from the scientific break-even experiments to ignition is consistent with an overall strategy that addresses the physics issues in achievable and responsible steps. TFTR provides a physics basis for plasmas in

a reactor-relevant regime with significant fusion power; CIT provides an analysis of plasma behavior when a significant amount of fusion power is absorbed in the plasma. The next challange after CIT is the development of an engineering machine (ITER) that incorporates the equipment for processing the fusion output power and producing electricity. The CIT experimental results provide the plasma database for the operating phase of the proposed ITER project.

To develop fusion as a viable alternative for energy production in the first half of the twenty-first century, the program's momentum must be sustained. The fusion program has experienced declining budgets since FY85, as shown in the following table.

BUDGET HISTORY OF FUSION*

Fiscal Year	Fusion Budget-$M*	% Change from Prior FY
FY'82	451.2	
FY'83	461.3	2.2
FY'84	466.0	1.5
FY'85	437.0	-6.6
FY'86	365.4	-16.4
FY'87	345.4	-5.5
FY'88	335.0	-3.0
FY'89 (proposed)	360.0	7.5

*All dollar values are in "as year" dollars

As shown, budgets to support the new initiative (Compact Ignition Tokamak and ALCATOR C-MOD), the operation of the Tokamak Fusion Test Reactor, the Advanced Toroidal Facility at ORNL, the DIII-D at GA Technologies, and the Microwave Tokamak experiment at Lawrence Livermore National Laboratory as well as other smaller experiments, the Development and Technology program, and the design work for the International Thermonuclear Experimental Reactor (ITER) will require an increase in FY89 and beyond. The Department of Energy FY 1989 Congressional budget request shows such an increase.

The OFE (DOE Office of Fusion Energy) financial guidance for CIT (shown in the following table) prolong the CIT schedule, with experimental operation beginning in FY 96 and DT operation in FY98.

OFE FINANCIAL GUIDANCE FOR CIT

FY'89	$27M
FY'90	$46M
FY'91	$64M
FY'92	$95M
FY'93	$101M

This schedule imposes a five-year interruption in front-line experimental research from the end of TFTR operations to the start of CIT operations. This 5-year gap is so long that continuity in and the momentum of the U.S. research program will be lost, the assembled research team may lose critical members, and the United States may slip from its position as a leader in the international fusion program.

The addition of near-term funding would accelerate this schedule and shrink the interval. We recognize that there is extraordinary competition for funds this year, but we ask the Subcommittee to review the magnetic fusion program results, its potential for providing an internationally distributed source of electric power and compare this promise with the competition. While it is difficult to predict the actual future energy requirements and environmental guidelines, the IEEE believes that it is important that the world of the twenty-first century have a number of technologies available for commercialization. Creating such an opportunity for choice should be the long-term driving force for developing fusion at an appropriate pace.

Over the past three decades, the American people have supported fusion with funding totalling $4.5 billion. The goal of scientific break-even is at hand and the achievement of ignition is on the horizon. In order to best capitalize on this investment and afford the opportunity of choice to the next generations, the fusion program should be sustained.

We in the United States currently have a strong scientific community that is on a par with that of other nations pursuing the fusion energy option. International collaboration appears to be a realistic possibility if our domestic program is sufficiently strong to attract other nations who are invited to collaborate with us. Hence, in this international arena, the IEEE in its own position paper on fusion power, dated November 24, 1985, states "the IEEE urges the continuance of broad areas of international cooperation in promoting fusion power research." We endorse open international scientific collaboration and support of a domestic program of size comparable to that of other nations.

In summary, the Institute suggests two measures:

- That the subcommittee examine and accelerate the proposed funding for CIT to retain the national technical staff which has been assembled and to sustain the momentum of the national program.

- The subcommittee should enable DOE to develop and execute a fusion program that involves both U.S. industry and international collaborators in an effort to provide fusion as an option for future generations.

OAK RIDGE NATIONAL LABORATORY
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April 11, 1988

Marilyn Lloyd
Chairman
Subcommittee on Energy
Research and Development
U. S. House of Representatives
Committee on Science, Space,
and Technology
Suite 2321 Rayburn House Office Building
Washington, DC 20515

Dear Madam Chairman:

Thank you for giving me the opportunity to contribute testimony on the Fusion Energy Program to your committee. My statement is attached. Let me add that your personal interest in the development of fusion is a continuing source of encouragement to myself and to my colleagues from across the country, who believe firmly that fusion is a critically important energy source for the future.

Sincerely,

John Sheffield

John Sheffield
Associate Director
for Confinement
Fusion Energy Division

JS:pf

Attachment

Statement of

John Sheffield

Associate Director for Confinement
Fusion Energy Division
Oak Ridge National Laboratory

to the

Subcommittee on Energy Research and Development
of the
House Science, Space and Technology Committee

April 8, 1988

Madam Chairman, thank you for giving me the opportunity to give written testimony on the Fusion Program to the Subcommittee on Energy Research and Development of the House Science, Space and Technology Committee. Last year, when I had the pleasure of addressing your Committee, I talked about the importance of collaboration in advancing fusion research and the improvements that had occurred in this area over the past decade. This year I will comment; on the development of important collaborative programs -the CIT and ITER; on the status and directions of TFTR, as you requested; on the role of ORNL, and on advances in our program.

At ORNL we are proud of having the most collaborative program in fusion, both nationally and internationally. During the past year; the Large Coil Task at Oak Ridge, which involved scientists and equipment from the European Community, Japan and Switzerland, was brought to a successful conclusion; we have either developed and constructed, or are preparing fueling or heating systems for a number of devices including TFTR, DIII-D, JET (EEC), Tore Supra (France); improved confinement devices, developed at ORNL are constructed, in construction, or in the design phase in Australia, Japan, and Spain.

When I gave testimony last year, I indicated that we were looking forward to applying our considerable strengths in support of design and development for the CIT and ITER. Earlier this year, we were disappointed in the diminished role of the Fusion Engineering Design Center and our technology programs in these activities. Recently, this situation seems to be improving and we are working actively with the lead laboratories, the Princeton Plasma Physics Laboratory (CIT) and Lawrence Livermore National Laboratory (ITER), to ensure that our capabilities are optimally utilized in these two important ventures.

We are very pleased at the initiative taken by the Department of Energy to increase the fusion budget. The United States is living mainly on past investments and is in danger of losing its leading position in this field, which is important not only to our energy security, but which is leading to very important spin-offs for industry - the recent development at ORNL in microwave sintering for ceramics will benefit a whole field of materials production. We support the CIT and ITER initiatives in the increased budget. We must, however, issue a note of caution. The proposed budget in the five year plan places very substantial constraints on the realization of the potential of existing facilities, and does not adequately fund technology development; even in those areas in which the U. S. has a clear lead, such as electron cyclotron frequency heating, fueling, and materials. I will return to the question of budget strategy below.

The Tokamak Fusion Test Reactor (TFTR) is the most visible element of our program. It has been successful in accomplishing a number of its primary goals. Notably, the simultaneous achievement of a high confinement parameter and a high ion temperature; 200 - 300 million degrees! It has been argued that, following this success, we should move on to the next phase of the program, deuterium-tritium (DT) operation, but in the more capable CIT device. For afficionados of the CIT, like myself, this approach has some appeal, however, it is unsatisfactory for a number of reasons. First, the program has invested a lot of money and effort in preparing TFTR for the D-T tests which will provide the first information on alpha-physics; second, it will qualify important diagnostics and technologies for the CIT; third, it will qualify the Princeton site for tritium handling. The principal difficulty at this time of restricted budgets is how to accomodate the delayed TFTR program, while launching the more ambitious CIT program. I have adopted the view, espoused by a number of my colleagues in the U. S. program, that the important 'Burning Plasma' element - TFTR and the CIT - should be undertaken as a roughly constant fraction of the national magnetic fusion budget. Thus, in the event we do not get the President's budget request of $360M in FY 1989 I would recommend postponing the CIT. Because, at this point, we would not be making cost effective use of our existing facilities, which must still fulfill their missions for fusion to advance in a coherent manner.

As to the relevance of the CIT, I agree with the view, expressed in the recent international review panel, that the CIT represents a substantial increase in capability over any existing facility, including the proposed upgraded versions of JET and JT-60. Thus it will help sustain the U.S. position in the international fusion program.

Finally, let me turn to the ORNL confinement program. As you know, we had proposed the Spherical Torus Experiment (STK), an innovative tokamak, to DOE and it had been well received. Regrettably, in this time of constrained funding, it was not possible for DOE to support it. In our main confinement program the Advanced Toroidal Facility (ATF) we have had great success. ATF was completed under cost in December, even though it was delayed by a year owing to late delivery of the vacuum vessel. During the past three months the experiment has been commissioned and the first plasma phase has been successfully completed. We are preparing to embark on an exciting phase of scientific discovery, for ATF has unique capabilities to understand the fundamental physics of toroidal plasmas, which is the primary route to developing a better magnetic confinmenmt device. It will also play a crucial complementary role as a part of the broader world stellarator program. It is unfortunate that budget constraints have successively lowered the ATF budget in recent years, slowing down the achievement of its important goals, and forcing us to reduce our involvement of the University community. We are hopeful that as ATF starts producing interesting scientific data, this trend will be reversed.

Madam chairman, let me end by saying that we at Oak Ridge join our colleagues from across the United States in expressing our continued appreciation of your support and interest in the Fusion Program.

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March 30, 1988

Dr. Rulon Linford, Program Director
Magnetic Fusion Energy
Los Alamos National Laboratory
Post Office Box 1663 Mail Stop F-646
Los Alamos, New Mexico 87545

Dear Dr. Lindford:

We are sorry that you could not participate in the hearing on the Fusion Energy Program which was scheduled for today. However, we are very interested in your views on the overall status of the programs including the major future thrust, the Compact Ignition Tokamak and international activity as on the International Thermonuclear Experimental Reactor. We are also interested in your views on the status and direction of the Tokamak Fusion Test Reactor including the proposed Deuterium Tritium burn.

Therefore, could you please submit a statement for the record on these subjects. Please send a copy directly to Dr. William Oran, Subcommittee on Energy Research and Development, B-374, Rayburn House Office Building, Washington, D.C. 20515.

Thank you for your help in this matter.

Sincerely,

MARILYN LLOYD
Chairman
Subcommittee on Energy
Research and Development

ML/Ola

Los Alamos
Los Alamos National Laboratory
Los Alamos, New Mexico 87545

DATE: April 27, 1988
IN REPLY REFER TO: MFE-88-96
MAIL STOP: F646
TELEPHONE: 505/667-7857

Dr. William Oran
Subcommittee on Energy Research and Development
B-274, Rayburn House Office Building
Washington, DC 20515

Dear Dr. Oran:

At the request of Marilyn Lloyd, I have prepared the enclosed statement expressing my views on the overall status of the Magnetic Fusion Energy program. A copy of her letter of March 10, 1988, is also enclosed.

Thank you for the opportunity to submit this statement for the record. If you have any questions, please feel free to contact me.

Sincerely,

Rulon K. Linford

Rulon K. Linford
Program Director
Magnetic Fusion Energy

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STATEMENT SUBMITTED TO THE SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT OF THE HOUSE SCIENCE, SPACE, AND TECHNOLOGY COMMITTEE

April 27, 1988
Rulon K. Linford
Program Director
Magnetic Fusion Energy
Los Alamos National Laboratory

Madam Chairman and members of the Subcommittee:

I appreciate the opportunity to express my views regarding the overall status of the Magnetic Fusion Energy (MFE) program. I will try to be responsive to the questions asked in the Chairman's letter to me dated March 30, 1988. In responding to these questions I have found it useful to refer to my testimony to your Subcommittee last year, and I have attached a copy of that testimony for easy reference.

National MFE Budget Projections and the CIT.

Last year I expressed my support for the Department of Energy's plan to construct a Compact Ignition Tokamak (CIT) but pointed out the need for increased budgets in future years in order to maintain the base program at the level of effort that existed at that time. Unfortunately, the national MFE budget dropped to $335M in FY88, instead of achieving the $345M level in the President's requested budget at that time, or the increased budget which was originally in the House Appropriations Bill. In addition, the President's requested budget of $360M for FY89 is followed by a flat level-of-effort projections for the years out through FY93. These flat projections do not contain the incremental funds I believe are necessary to construct the CIT; the $360M for FY89 is only a cost-of-living increase over the $345M budget I was anticipating for FY88 during my testimony last year.

In light of these budgetary changes during this past year and the continuing constraints on the Federal budget, my position with regard to the budget and the construction of CIT can be summarized as follows:

- The study of burning plasma in the ignited state remains the next major step to be taken in the MFE program.

- I support the $360M in the President's request as the minimum increase (over the present $335M) required to continue with a stretched-out construction of the CIT. However, incremental funding is required in FY90 and beyond in order to construct the CIT on a reasonable schedule while maintaining program vitality.

Program Vitality and Balance, and International Cooperation.

Chairman Lloyd's letter asked me to comment on three specific topics: the use of DT in TFTR, the CIT, and the ITER. These three activities constitute the main elements in one of the four key technical issues called out in the Magnetic Fusion Energy Program Plan. This issue of burning plasmas was described in Dr. James Decker's testimony to this Subcommittee, along with the other three key technical issues: magnetic confinement systems, materials, and nuclear technology. As he pointed out, the "Program's goal is to establish the scientific and technological base for fusion energy early in the next century," and "work on each of (these) four key technical issues" is required in order to meet that goal. In my view, however, the budgetary constraints prevent us from pursuing these key issues in parallel with

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sufficient strength to meet that goal in the early part of the next century, even though international collaboration is used to the maximum extent possible in order to achieve the goal in a timely and cost-effective way.

Equating progress in the fusion program solely with progress in the burning plasma area is a common and easily made mistake. Burning plasma studies involve the largest and most expensive facilities, and the goals of breakeven and ignition are essential, easily measured, and easily understood. The excitement that is generated by progress toward these goals is understandable and justified. Although the achievement of these goals is essential, it is by no means sufficient to ensure the success of fusion as an energy source. As pointed out in the recently completed OTA report, fusion must provide a superior product in the areas of economics, environment, and safety if it is to successfully compete in the energy market of the future. Progress in the other three key areas is essential to be able to assess whether fusion could provide that superior product.

In an effort to maintain an appropriate balance between the four key issues, within the projected flat budgets, the President's projected budget includes the stretched-out construction of CIT and a slower pace for the other main elements of the program. The slower pace makes it more difficult to contribute on a par in the international community, and unfortunately counters the past efforts to coordinate program plans to maximize the complementary output from the international community. The slower pace also leads to less efficient use of our national resources, both personnel and facilities. The result is that small increases in the national budget would have very high leverage in increasing the productivity and maintaining the vitality and progress that has earmarked the MFE program in the past.

International Thermonuclear Experimental Reactor (ITER) Design and R&D

The ITER process is producing an important focus for a variety of ongoing R&D activities in the national program, and it has the potential for leading toward even improved international cooperation and effectiveness in these activities. The process should point out needs and begin focusing resources on achieving the physics and technological improvements needed for the experimental step beyond the CIT.

DT Experiments in TFTR

I concur with the MFAC assessment (charge number 17) that important scientific information would accrue from using DT in TFTR if the expected improvements in the operation of TFTR are forthcoming during the next nine months. The increased heating power available on TFTR during that period is projected to yield parameters consistent with values of Q between 0.5 and 1 if DT were used. The subsequent operation of TFTR with DT would not only result in important scientific information regarding alpha-particle physics but would achieve one of the major milestones for magnetic fusion.

Major Fusion Activities at Los Alamos

During the past year we have successfully operated the Tritium Systems Test Assembly (TSTA) at its full design performance of 100 grams of tritium inventory and a kilogram per day processing rate through the complete tritium fuel loop. The extremely successful separation of impurity gases deliberately injected into the fuel loop, and the successful containment and control of the tritium under a variety of operating conditions are important achievements for the fusion program and for the future use of tritium in TFTR, CIT, the ITER design activity, and follow-on experiments. This work has been carried out in collaboration with the Japanese.

Progress has also been made in the construction of the Confinement Physics Research Facility (CPRF) at Los Alamos which will provide the opportunity of studying the next step in the Reversed Field Pinch (RFP), and other compact fusion concepts in the future. A surplus 1430-MVA generator was obtained from the Tennessee Valley Authority and was transported from Tennessee to Los Alamos. This cost-effective acquisition of this surplus equipment is vital to CPRF. The generator will be capable for supplying 600 MJ of energy to the experiment. In the future, the deliverable energy could be increased to 2 GJ with the addition of a flywheel.

Last fall as the project progressed into the early construction phase, we recognized that the budget level of $72.5M was insufficient for completion of the full 4 MA CPRF and associated RFP experiment called ZTH. To prevent additional pressures on the MFE budget, the project has been descoped to an initial operation of 2 MA. The total cost of the descoped CPRF has been peer reviewed and a new data sheet should be submitted in the near future. If it is found to be appropriate, the facility could be efficiently upgraded to 4 MA after initial operation at 2 MA.

Although this descoped CPRF is not as large a step as we had originally planned, it is still responsive to the international RFP program. The only other device comparable to the ZTH is the RFX device being constructed in Padova, Italy. The RFX will also have a 2 MA capability and is planned to be operational in late 1989. Although the ZTH experiment is not planned to become operational for three years after RFX (partially due to a delay mandated by the flat budget projections), ZTH contains important features which make it an appropriate and complementary facility in the international RFP program. It has been designed for an efficient upgrade capability to 4 MA and to provide ohmic heating power densities that are an order of magnitude greater than that of RFX. The design of the ZTH machine allows the addition of impurity control measures such as divertors. In addition, it is designed to have a plasma duration long compared to the conducting shell time, which will allow us to explore equilibrium control and stabilization regimes not accessible by the present design of RFX. However, the early operation of RFX will allow us to become experienced with RFPs in the higher current and higher temperature regimes made possible by these two new devices. These and other complementary features of the two machines form the basis of international agreements being developed and implemented at the present time.

As I described more fully in my testimony last year, these international studies on the RFP are important in extending the understanding of RFP physics and scaling laws into the kilovolt temperature regime. These studies should lead to improved understanding of MHD behavior of high-beta, toroidally symmetric systems for the benefit of the overall fusion program. In addition, if the scaling laws remain valid into the ignition regime, the RFP could lead to a reactor that would not require auxiliary heating or superconducting technologies, resulting in a simpler, smaller, and lower cost device.

The RFP program is just one example of both ongoing and proposed areas of research in the national program that could significantly improve the potential of fusion to contribute to the world energy supply of the future.

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TOM MCMILLEN, Maryland
DAVID E. PRICE, North Carolina
DAVID R. NAGLE, Iowa
JIMMY HAYES, Louisiana
DAVID E. SKAGGS, Colorado
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MANUEL LUJAN, JR., New Mexico
ROBERT S. WALKER, Pennsylvania
F. JAMES SENSENBRENNER, JR., Wisconsin
CLAUDINE SCHNEIDER, Rhode Island
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TOM LEWIS, Florida
DON RITTER, Pennsylvania
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RON PACKARD, California
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H.A. OLIN W. FAWELL, Illinois
D. FRENCH SLAUGHTER, JR., Virginia
LAMAR SMITH, Texas
ERNEST L. KONNYU, California
JACK BUECHNER, Missouri
JOEL HEFLEY, Colorado
CONSTANCE A. MORELLA, Maryland
CHRISTOPHER SHAYS, Connecticut

HAROLD P. HANSON
Executive Director
ROBERT C. KETCHAM
General Counsel
R. THOMAS WEIMER
Republican Staff Director

U.S. HOUSE OF REPRESENTATIVES
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY
SUITE 2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515
(202) 225-6371

March 29, 1988

Dr. James F. Decker
Acting Director
Office of Energy Research
Department of Energy
Washington, D.C. 20585

Dear Dr. Decker:

Thank you for your letter of March 14, 1988 describing a five year plan to build the Compact Ignition Tokamak (CIT) within current budgetary resources. The Subcommittee also believes that CIT is crucial to the development of the U.S. fusion program.

I understand that the recent review of the CIT design by a non-DOE, non-Princeton "peer" group has resulted in a significant design change to the CIT. This design change appears to increases the potential capabilities of the CIT to obtain a fully ignited, self-sustained burn. I applaud these efforts and the foresight of both DOE and Princeton to use an external review. The use of such groups should be continued in future reviews.

Again, the Subcommittee feels that the mission of CIT is crucial to the U.S. program, and that, within reason, cost should not be considered a constraint. Please send me the details on any additional parameter and estimated cost changes which in your judgement would substantially enhance the probability of developing a burning plasma.

I would also appreciate additional information on the TFTR program; namely, the relationship and/or difference between the higher Q, D-T experiments to be performed on TFTR and those proposed for Joint European Torus (JET) which would be conducted in roughly the same time frame.

DOE should be commended for its efforts to revise the program/budget plan of the fusion activities in the context of the overall deficit of the United States. However, because of the critical nature of CIT, every reasonable effort should be made to assure its success.

Sincerely,

MARILYN LLOYD
Chairman
Subcommittee on Energy
Research and Development

ML/Ola
cc: The Honorable Sid Morrison

Department of Energy
Washington, D.C. 20585

March 14, 1988

Honorable Marilyn Lloyd
Chairman, Subcommittee on Energy
Research and Development
Committee on Science, Space and
Technology
House of Representatives
Washington, D.C. 20515

Dear Madam Chairman:

The FY 1988 Appropriation Act directed the Department of Energy to submit a five-year plan showing how the Compact Ignition Tokamak (CIT) could be built while completing experiments on the Tokamak Fusion Test Reactor (TFTR) within current budgetary resources. The Department has prepared such a plan, allowing the CIT to begin operation in 1996 while completing the TFTR program as planned and maintaining a balanced but constrained base program. Our international commitments would be honored, but work aimed at issues beyond TFTR and CIT would proceed more slowly or be postponed, as appropriate.

The Department believes that CIT is critical to the development of fusion energy and to the U.S. fusion program. It is the only deuterium-tritium experiment proposed for the period between the TFTR in the U.S. (and JET in the European Community) and a future engineering test reactor. Accordingly, CIT is the only proposed experiment in the world-wide program aimed at addressing the most fundamental scientific issue for fusion, that of understanding burning plasma physics. It is also the natural and cost-effective follow-on to the TFTR program.

The Department also believes that the TFTR program must be completed as planned. Both the science and the technology of the TFTR D-T program are considered necessary predecessors to the CIT program.

However, the resource requirements related to the Department's original schedule for construction of CIT would, in a flat budget scenario, result in unacceptable damage to the fusion program base. On the other hand, waiting for the completion of TFTR to begin CIT would result in an unacceptable gap in D-T experiments in the U.S. The problem associated with this gap is both a programmatic one for the U.S. and its collaborators in the international arena and an institutional one for Princeton Plasma Physics Laboratory (PPPL).

The Department's plan is, therefore, a trade-off between the requirements of the CIT and the balance of the remaining program. The CIT project has been stretched out so that operation would begin in 1996, and the rest of the fusion program has been limited to only essential activities, especially in 1990-92. The attached tables show the details. The first, summary level table is included in the FY 1989 budget justification. The second, more detailed table is included for your convenience. The major features of this plan are discussed below.

In the Confinement Systems program, the PBX at PPPL is put on stand-by for a little over two years, while PPPL concentrates its efforts on TFTR and CIT. The Lawrence Livermore National Laboratory (LLNL) experimental program continues at a reduced level through 1991 and then will be merged with the GA program, centered around the D-III-D facility. Alcator C-Mod at the Massachusetts Institute of Technology and the Advanced Toroidal Facility at Oak Ridge National Laboratory will continue, but any future upgrades will be postponed. International collaboration continues, as planned, as an integral part of the U.S. program. The mirror program, consisting of the completion and mothballing of MFTF at LLNL, will be completed as of 1990.

In the Development and Technology Program, participation in the Presidential initiative to design and conduct validating research and development for the International Thermonuclear Experimental Reactor (ITER) through a four-party international collaboration is supported at the agreed upon level. Basic research and development on systems needed for CIT and other near-term experiments will be maintained at a minimum acceptable level. Long-range technology, such as materials and nuclear technology, will continue at the basic research level, with a strong element of international collaboration but with no major facilities planned.

In the Applied Plasma Physics program, the advanced concept experiments will be continued until completion but with considerably slower pacing; new initiatives in the tokamak improvement program will be postponed.

In summary, this plan focuses the U.S. fusion program strongly on ignition physics issues based on TFTR and CIT. It maintains a limited number of recent initiatives aimed at improved toroidal confinement and maintains continued awareness programs in long-range technology. International commitments, including participation in ITER, will all be met under this scenario. The tightness of this budget plan begins to be relieved in 1992 and can be expected to improve each year thereafter. The Department has considered the requirements of both a balanced base program and preparation for a program for the future and believes this plan meets those requirements, recognizing Federal budget stringencies.

Sincerely,

James F. Decker
Acting Director
Office of Energy Research

Enclosure

cc:
Honorable Sid Morrison
Ranking Minority Member
Subcommittee on Energy Research
and Development
Committee on Science, Space
and Technology
House of Representatives
Washington, D.C. 20515

APPENDIX 2

Department of Energy
Washington, DC 20585

May 26, 1988

Honorable Marilyn Lloyd
Chairman
Subcommittee on Energy Research
and Development
Committee on Science, Space, and Technology
House of Representatives
Washington, D.C. 20515

Dear Madam Chairman:

On March 30, 1988, Dr. James F. Decker, Acting Director, Office of Energy Research appeared before your subcommittee to discuss the Department's Fiscal Year 1989 budget authorizaton for Energy Research.

Following the hearing, you submitted written questions for our response to supplement the record. Enclosed are the answers to those questions, which also have been sent to the committee staff.

If you have any questions, please have your staff call Rebecca Thompson on 586-4277. She will be happy to assist.

Sincerely,

for Rebecca A. Thompson
Robert G. Rabben
Assistant General Counsel
for Legislation

Enclosures

POST-HEARING QUESTIONS AND ANSWERS

RELATING TO THE

MARCH 30, 1988 HEARING

BEFORE THE

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

OF THE

COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

HOUSE OF REPRESENTATIVES

Magnetic Fusion Energy Program

Question 1: The Plasma Fusion Theory and Experimental Plasma Research activities under the Applied Plasma Physics budget line item appear to be the incubator subprograms that will result in new and innovative ideas which might dramatically advance fusion.

- What percentage of funds in these areas go to universities, which are generally thought to be the wellspring of new ideas in the country?

- What provisions are there for bringing in new groups (i.e., fresh blood with new ideas) into this part of the program?

Answer: The percentage of Plasma Fusion Theory funds supporting work at universities in FY 1987, FY 1988, and FY 1989 is 36%, 34%, and 37%, respectively; for the Experimental Plasma Research line these percentages are 77%, 78%, and 79%. Each year, in these areas, a few research programs are completed and discontinued so that a few new programs can be initiated. Currently, we receive and consider, on an equal basis, about 30 new proposals from laboratories and universities each year. This number includes separate, new proposals from current contractors and grantees, as well as from new groups. About one fourth of these new proposals are funded.

Magnetic Fusion Energy Program

Question 2: The fusion plasma theory budget essentially remains flat from 1987 to 1989. Yet you have apparently initiated a theoretical effort to support CIT.

- How much in your theory budget will support CIT related activities in FY 1989?

- What areas have been de-emphasized and how critical are they?

Answer: In FY 1989, about $7.0 million of the total Fusion Plasma Theory budget of $18.8 million will fund activities that support the development of CIT. These CIT-related activities also support other projects and concept advancement that require the same basic physics. More than one half of these funded activities supporting CIT are qualitatively the same research as prior to FY 1987, but now include calculations specifically for CIT parameters. Since FY 1987, the mirror theory funding has nearly disappeared from the Fusion Plasma Theory budget ($2.3 million in FY 1987 to $0.1 million in FY 1988), and actual dollar decreases of about $0.1 million each have also occurred in alternate concept theory and atomic theory. These decreases in theory support correspond to discontinued or slowed experimental activities and are, therefore, consistent with overall program priorities.

TFTR

Question 1: What new scientific information is expected to be obtained by conducting deuterium-tritium (D-T) experiments in TFTR that could not be so obtained by using deuterium alone, or by using tritium alone?

Answer: The introduction of deuterium-tritium into TFTR will allow the first exploration of the impact of sizable quantities of alpha particles. In 1987 the DOE requested the MFAC to address the question of what information would be gained by doing the D-T experiments in TFTR. Their conclusion was: the information that will be gained from such experiments can be broken into two categories -- single particle effects and collective effects. Single particle effects involve how a single alpha particle will interact with the background plasma and magnetic fields. TFTR will generate enough alphas, even at the present performance level, to be able to address these effects. The collective effects are those effects where the alpha particles as a group can disturb the background plasma and themselves. At full performance, TFTR should be able to address these issues in a limited manner, but it will require an ignition experiment such as CIT to fully address these physics issues. The experience in TFTR with active tritium systems, remote maintenance, neutron hardened diagnostics, and single particle alpha effects will contribute to the effective design and operation of CIT.

TFTR

Question 2: Why are D-T experiments in TFTR necessary for the subsequent design and operation of the Compact Ignition Tokamak? Why wouldn't T-T experiments suffice to obtain experience with the tritium delivery and cleanup systems, etc.?

Answer: A significant amount of information concerning the operation of the tritium storage and delivery systems, the operation of the tritium cleanup systems, and the amount of tritium retained in in-vessel components could indeed be gained from T-T operation. However, this is only part of the information to be gained from D-T experiments. Important additional information on alpha particle physics and operation of a tokamak in a high radiation environment will be gained only from the planned D-T operation. TFTR will provide the first information on alpha particle confinement and a direct test of diagnostics designed to measure the alpha particle distribution in the plasma and escaping from the plasma. In addition, it will provide experience for CIT in measuring plasma parameters such as plasma density and temperature in the presence of high levels of neutron and gamma radiation. Finally, it will demonstrate for the first time how to operate newly developed remote maintenance equipment as well as "standard" tokamak systems such as vacuum systems, control systems, and auxiliary heating systems in a D-T environment.

TFTR

Question 3: Professor Bruno Coppi of MIT has suggested the use of deuterium-helium 3 ($D\text{-}He^3$) mixtures in TFTR to reduce the production of neutrons relative to those produced in D-T experiments. What would be the advantages and/or disadvantages of $D\text{-}He^3$ experiments versus D-T experiments in TFTR?

Answer: The production of significant fusion reactions using a deuterium-helium mixture requires substantially higher plasma and machine parameters than TFTR will be able to achieve. In Prof. Coppi's written testimony he indicated that achieving these parameters would require an Advanced Ignition Experiment (AIX), which had capabilities even beyond those planned for CIT. As an alternative, $D\text{-}He^3$ can be used with the ICRF heating system on TFTR to simulate the physics of deuterium-tritium fusion products. These and other simulation experiments are already planned for the pre-D-T phase of TFTR. These modeling experiments, however, cannot provide the full range of experience to be found in the actual D-T experiments on TFTR.

TFTR

Question 4: What would be the additional expense of operating and decommissioning TFTR if D-T experiments are conducted versus the costs involved with D-D, D-He^3 or T-T experiments alone?

Answer: With respect to operating costs, approximately $25.5 million and $24.7 million are budgeted specifically for D-T preparation in FY 1989 and FY 1990 respectively. Only a small amount of this funding would be required for continued D-D and D-He^3 operation; however, the majority of it would be required even for T-T operation alone. Also, a significant (about 30%) amount of the D-T preparation work on TFTR contributes directly to CIT. For example, the tritium handling systems and many of the radiation monitors now being commissioned for TFTR will also be used for the CIT. With respect to decommissioning, it is clear that decommissioning following continued D-D and D-He^3 operation would be modest. Decommissioning has always been a recognized part of the D-T program; however, only preliminary planning has been carried out to date. As a result, detailed cost estimates for the complete decommissioning following D-T or T-T operation are not available at this time. With the D-T program in TFTR scheduled to begin in late 1990 and continue into 1991, the Office of Fusion Energy has requested from Princeton Plasma Physics Laboratory a detailed plan for the complete decommissioning of TFTR. Funding has been included in the five-year plan recently submitted to Congress for the initial decommissioning activities in 1992 and 1993. We expect additional costs for decommissioning in future years, with some flexibility according to how long the facility sits before being dismantled.

International Thermonuclear Experimental Reactor (ITER)

Question #5: What is the agreed-upon management structure for and location and duration of the International Thermonuclear Experimental Reactor (ITER) conceptual design effort?

Answer: Each of the four ITER Parties (the European Community, Japan, U.S.S.R, and U.S.) has agreed to participate according to the ITER Terms of Reference. The Terms of Reference specify the management structure, duration, and key milestones, as well as the objectives for the design activity. Under the Terms of Reference, the ITER Council, composed of two representatives from each Party, is responsible for the overall direction of the ITER Activity and exercises overall supervision of its execution. The ITER Management Committee, composed of one member from each Party, is responsible for the conduct of the Conceptual Design Activities, including validating R&D, within the overall directions provided by the Council. The Council is advised by the ITER Scientific and Technical Advisory Committee, composed of three members from each Party.

The ITER Activity begins in April, 1988 and will conclude on December 31, 1990. Throughout the Activity, each Party will perform coordinated tasks for ITER at its home institutions and will participate in joint work at a common site. During the Definition Phase, April-November, 1988, joint work is to be done at the Institute for Plasma Physics in Garching, West Germany. At the conclusion of the Definition Phase, the Council will select a joint work site for the Design Phase, which will occupy the remainder of the three-year Activity.

International Thermonuclear Experimental Reactor (ITER)

Question #6: What amount of funding in the FY 1989 budget request is for the ITER activity? What funding over what period of time will be required to complete this effort?

Answer: The FY 1989 budget request contains $8 million for conceptual design and $8 million for research and development to validate key aspects of the design. This is the level of funding being spent in FY 1988 and also is the level planned for FY 1990. As specified in the Terms of Reference adopted by the four ITER Parties, each Party shall contribute equally at this level.

International Thermonuclear Experimental Reactor (ITER)

Question #7: What new R&D efforts are being undertaken by the U.S. Magnetic Fusion Energy Program in support of ITER that would not be conducted in the absence of such an activity, and what funding is requested for these efforts?

Answer: Before ITER, the major world fusion programs, including the U.S., were devoting a portion of their efforts to studies and long-range research and development to prepare for an engineering test facility. Information was exchanged between programs, but there was little coordination of work. The ITER Activity evolved from a world-wide technical consensus on the need and general objectives for such a facility. Consequently, the objectives of ITER are similar to those identified in the earlier U.S. effort. The specific ITER-funded activities represent extensions of program activities to satisfy particular needs of the design. Further adjustments in the U.S. effort may be needed as the ITER design evolves and as negotiations on a coordinated work plan continue among the Parties.

COMPACT IGNITION TOKAMAK (CIT)

Question 8: What is the level of international collaboration/participation expected for the CIT?

Answer: The international fusion community has participated in the formulation of CIT objectives. Leading scientists from U.S.S.R., Japan, and Europe have strongly endorsed the project and urged the earliest possible construction and experimental schedule. We do not, however, expect substantial foreign cost-sharing on CIT. CIT is the logical successor to TFTR and is perceived as a U.S. responsibility. Europe and Japan have both made very substantial investments in new experimental tokamak facilities over the past 10 years and will be funding the operation of these experiments during the time we are building CIT. CIT foreign participation will likely consist of personnel assignments during design and operation. It is also possible that some hardware components or experimental diagnostics will be supplied by other nations to enhance CIT program effectiveness.

COMPACT IGNITION TOKAMAK (CIT)

Question 9: What is the status of the Environmental Assessment for CIT sited at Princeton Plasma Physics Laboratory? Will an Environmental Impact Statement be required?

Answer: PPPL, with assistance from environmental and safety experts from INEL, has prepared an analysis of the environmental effects associated with CIT, and the responsible DOE field office is nearing completion of a draft Environmental Assessment that will be subjected to a thorough review by the Office of Environment, Safety, and Health at DOE. In accordance with regulations, the conclusions of the Department's review of the Environmental Assessment will determine whether an Environmental Impact Statement will be required. In addition, Princeton University is independently reviewing the environmental aspects of CIT. So far, all preliminary indications are that the environmental effects of CIT will be similar to those of TFTR.

COMPACT IGNITION TOKAMAK (CIT)

Question 10: How is DOE working with the State of New Jersey and local governmental and civic entities to address any radiological concerns of the CIT?

Answer: DOE is nearing completion of the draft CIT Environmental Assessment. As part of the review process for this document, copies will be submitted for comment to the State of New Jersey and other governmental entities, and made available for comment to members of the public. In addition, Princeton Plasma Physics Laboratory has been working extensively with the local community to acquaint them with the effects of CIT on the environment.

COMPACT IGNITION TOKAMAK (CIT)

Question 11: What are the terms of DOE's 40-year lease with Princeton University? When does this lease expire?

Answer: The basic terms of DOE's lease with Princeton University are that DOE will have access to and use of the C and D sites on Princeton's Forrestal Campus for magnetic fusion research and related activities and that DOE will pay Princeton University 90% of the annual net fair rental value established every 5 years through a mutually acceptable appraisal process. The current annual payment is $1.771 million. The lease expires on October 1, 2026.

COMPACT IGNITION TOKAMAK (CIT)

Question 12: Since our hearings last year, the completion date of the CIT has slipped by 2 1/4 years, the CIT's total cost estimate has increased by $69 million (from $357.0 million to $426 million), and the total project cost has increased by $112.01 million (from $444.5 million to $556.51 million). Please explain the large increase in costs.

Answer: In order to maintain a balanced base fusion program and remain within the current fusion budgetary resources, it has been necessary to reduce the previously planned CIT construction funding in FY 1989, 1990, and 1991. This, in turn, has resulted in an extension in the CIT project completion date of about 2 1/4 years to 1st quarter FY 1996. The increase in costs is due to added escalation and the need to retain the core project staff for the longer construction period.

Magnetic Fusion Energy Program

Question 13: What is the status of the Confinement Physics Research Facility (CPRF) being fabricated at Los Alamos National Laboratory with regard to project scope, costs, design, etc?

Answer: In accord with the FY89 budget request to Congress, work is proceeding on the Confinement Physics Research Facility. The motor generator is scheduled to arrive at the Los Alamos site in April 1988, and all of the coils are being fabricated. At present, project scope revisions are being considered by DOE. An FY89 data sheet identifying specific revisions will be prepared and submitted to Congress shortly. Costs to date have been about $16 million.

Magnetic Fusion Energy Program

Question 14: Please provide a chronology of events that have led to the current impasse between the DOE and OMB on the funding and scope of CPRF. What is being done to resolve the impasse?

Answer: From 1982 to late 1985, DOE planned the fabrication of a Reversed Field Pinch (RFP) device with plasma current capability in the range of 0.6 to 2.0 MA. In early 1986, based upon LANL recommendations, industry overtures to share costs, and peer review, DOE proposed to build a Confinement Physics Research Facility (CPRF) at a cost of $52 million, with the expectation that industry would fund the first device in the facility. CPRF was to be a facility in which fusion research could be conducted on any of the compact fusion concepts in a cost effective way. The first device to go into the facility was to be a 4 MA Reversed Field Pinch supplied by industry. One year later, in early 1987, after soliciting industry participation and receiving no acceptable proposals, DOE proposed, in the FY88 budget, full government funding of $72 million for the facility and the RFP device. By the end of 1987, it became clear that LANL was experiencing CPRF cost problems that would lead to a major overrun of the total project cost unless the scope was reduced. Scope changes are under review and will be reflected in a revised FY89 data sheet that will be submitted to Congress shortly.

Device and Facility Funding

Question 15. Please provide actual FY 1987, and estimated FY 1988 and FY 1989 funding for the following devices. Please include all sources of funding, including operating, capital equipment, and GPP: (a)TFTR, (b) PBX, (c) ATF, (d) Doublet-D-III,(e) Alcator C Modification, (f) International Fusion Superconducting Magnet Test Facility, (g) Plasma Materials Test Facility, (h) Tritium System Test Assembly, (i) Radiofrequency Test Facility, (j) RTNS-II, (k) CPRF, including the ST-40m, (l) Maryland Spheromak, (m) LSX, (n) TEXT, (o) TARA, (p) MFTF-B, (q) TMX-U, (r) Phaedrus, (s) MST, (t) Reversatron, (u) S-1, (v) FRX-C, (w) Constance, and (x) Lamex.

Answer:

(Dollars in Millions)

	FY 1987	FY 1988	FY 1989
a. Tokamak Fusion Test Reactor (TFTR)	75.7	72.2	74.3
b. Princeton Beta Experiment (PBX)	11.5	11.1	11.4
c. Advanced Toroidal Facility (ATF)	19.1	18.1	18.0
d. Doublet-D-III	33.9	29.6	32.5
e. Alcator C-Modification (C-MOD)	8.5	10.1	15.5
f. International Fusion Superconducting Magnet Test Facility (IFSMTF)	7.5	1.0	0
g. Plasma Materials Test Facility	0.9	1.0	1.0
h. Tritium Systems Test Assembly (TSTA)	2.0	2.0	2.0
i. Radiofrequency Test Facility (RFTF)	1.0	1.1	1.1
j. RTNS-II	0.8	0	0.3
k. CPRF/ZT-40M	14.3	12.8	15.9
l. Maryland Spheromak	0.4	0.8	0.8
m. LSX	3.0	3.9	3.3
n. TEXT	3.7	3.6	4.3
o. TARA	6.2	0.4	0
p. MFTF-B	10.5	2.9	3.0
q. TMX-U	2.0	0	0

(Dollars in Millions)

	FY 1987	FY 1988	FY 1989
r. Phaedrus	1.7	1.6	1.7
s. MST	0.7	0.9	1.1
t. Reversatron	0.1	0.1	0.1
u. S-1	3.0	0	0
v. FRX-C	2.9	3.7	3.3
w. Constance	0.3	0.3	0.3
x. Lamex	0.4	0.3	0.2

INTERNATIONAL COOPERATION

Question 16: Please provide actual FY 1987, and estimated FY 1988 and FY 1989 funding for research at foreign facilities including the following:

a. JET
b. JT-60
c. Tore Supra
d. ASDEX

Answer: Funding of U.S. fusion research at foreign facilities is estimated as follows:

(Dollars in Millions)

	FY 1987	FY 1988	FY 1989
a. JET	$1.1	$1.1	$0.7
b. JT-60	0.2	0.2	0.2
c. Tore Supra	3.5	4.2	2.9
d. ASDEX	0.4	0.5	0.6

This funding supports U.S. research teams working on specialized auxiliary and diagnostic systems for application to the above tokamaks both in the U.S. and directly at the foreign sites. In turn, the U.S. receives experimental access and programmatic input on unique foreign facilities with capital costs in the hundreds of millions of dollars.

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Hearing on the Department of Energy (DOE)
FY 1989 Authorization Request for the
Magnetic Fusion Energy Program

Additional Questions for the Record
Dr. Harold P. Furth, Director
Princeton Plasma Physics Laboratory

1. The CIT will be the premier machine in the US Fusion Program in the 1990's.

   a. What provision have you made to involve other US fusion groups in your program?

   The CIT involves more of the US fusion community within a single enterprise than any previous fusion project. Major partners in CIT include: Oak Ridge National Laboratory, Massachusetts Institute of Technology, Idaho National Engineering Laboratory, Los Alamos National Laboratory, and General Atomics. Also contributing to the CIT effort are: Sandia National Laboratories, University of Texas, Fusion Research Center, and Lawrence Livermore National Laboratory. Additional contributions to CIT are being made in physics and engineering activities, as well as on advisory and review panels, by individuals representing virtually every major center of fusion research in the US.

   b. What provisions have you made to involve foreign groups? Is there any documentation on the resources they might contribute?

   Foreign fusion research groups and individuals have been involved in CIT from its inception. There have been extensive informal discussions with the European fusion community concerning the desirability of an integrated D-T-burning program, starting with the D-T phase on TFTR and proceeding to JET, then CIT, and finally ITER or NET. A two-way exchange of research personnel has been agreed upon in principle, and possibilities for more extensive exchanges are under discussion.

The Soviet Union has expressed strong interest in becoming a major supporter of CIT. In a series of meetings at Princeton in January 1988, an agreement was made to continue discussion toward a possible collaboration. A US delegation from CIT will be in the USSR during the week of June 12-18, 1988 for detailed discussions of possible contributions in hardware that the Soviets could make to CIT, in exchange for the opportunity to participate in the experimental program.

Canada has expressed specific interest in contributing to the remote-handling task on CIT. Informal discussions have also been held with the Japanese, but because of timing considerations, they would seem to be more interested in D-T experiments on TFTR than in CIT participation.

2. The new CIT schedule proposed by DOE shows a gap in operation between the finish of TFTR and the beginning of CIT. Do you foresee major problems with loss of skilled manpower? For example, how many of your 600 employees at TFTR might have to be let go?

The Laboratory now has a work force of slightly less than 1,000 full-time employees, supplemented by about 100-200 subcontractor and temporary personnel. During the interval between TFTR and CIT operation ('92-'96), a major part of the Laboratory's staff will be needed for the CIT construction effort. We also plan to operate PBX-M and a few very small research devices, as well as carrying out theoretical studies and some projects in fusion-related areas of plasma physics. The level of the Laboratory work force required during this period is slightly below the present number of permanent employees, but we expect to reach that level by means of normal attrition. Beginning in '96, the Laboratory work force will return to its present level.

Following completion of the TFTR experiments, part of the TFTR team will be needed at PPPL to prepare for experimental operations on CIT. Arrangements have been made with other major tokamak laboratories for long-term exchanges of personnel, with a substantial part of the TFTR team expected to visit JET and JT-60 during the period '92-'95.

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Hearing on the Department of Energy (DOE)
FY 1989 Authorization Request for the
Magnetic Fusion Energy Program

Republican Member Questions for the Record
for the Hearing of March 30, 1988, for
Dr. Harold P. Furth, Director
Princeton Plasma Physics Laboratory

1. How much has been invested in the capability of TFTR to operate with D-T, and how much remains to be invested?

   The total investment to date in the TFTR D-T capability is $317M. An additional $56M will be spent on D-T-related hardware to implement our plan for the TFTR break-even demonstration.

2. What is the operating cost of TFTR relative to that of other large tokamaks, such as JET and JT-60?

   The FY'88 budget for TFTR is $72M, while the JET budget is 105 MECU (about $120M) and the JT-60 budget is 20 billion yen (about $160M). The ratios of these three budgets have remained roughly the same during the past three years.

3. What would be the impact on the PBX-M program of the proposed shut-down during FY 1990 and FY 1991?

   The PBX-M device has just reentered experimental operation with enhanced capabilities for plasma shaping, plasma confinement, and suppression of instability modes that were found to limit the performance (beta-value) of the earlier PBX device. Radio-frequency heating will be added during FY'89 to provide better control of the plasma pressure profile, but a special radio-frequency system for current-profile control cannot become operational until the end of FY'89, i.e., just before the proposed shutdown.

The suspension of PBX-M operations during FY'90-91 would seriously disrupt experimental work on the most promising approach to the realization of very-high-beta tokamak configurations -- which are of prime interest for the development of economically attractive fusion power.

4. What could the CIT project do with an added $15 million in FY 1989?

The CIT Project has just submitted to DOE its field task proposals for FY'89 and '90. Our FY'89 proposal includes two requests for incremental funding: (1) $6.0M of operating funds to accelerate the CIT R&D efforts; (2) $9.0M of PACE funds to accelerate the design process. If these incremental funds can be made available, we estimate that the date of CIT first plasma can be advanced by about five months.

5. You state in your testimony that "an actual policy of trying to raise the entire CIT construction cost from within the existing program would be quite problematical. Such an approach tends to subject the CIT construction schedule and the MFE operating budgets to levels of pressure that seem to go beyond the point of cost-effectiveness." What alternative Magnetic Fusion Energy Program would you propose for the next five years?

The most productive approach would be to follow the proposed OFE program, but with a budget that grows slightly in real dollars. After reaching $360M in FY'89, the budget should rise to $380-390M in FY'89 dollars. The CIT construction schedule could then be shortened, so as to reduce the gap between TFTR and CIT operation, and the rest of the magnetic fusion program would also be able to proceed in a vigorous and cost-effective way.

6. Professor Bruno Coppi of MIT has suggested the use of deuterium-helium 3 ($D-He^3$) mixtures in TFTR to reduce the production of neutrons relative to those produced in D-T experiments. What would be the advantages and/or disadvantages of $D-He^3$ experiments versus D-T experiments in TFTR?

The main advantage of D-T over $D-He^3$ fusion fuel is that is burns much more readily. When D-T fuel is used in the TFTR, we expect to reach an energetic alpha-particle population fraction up to 1% of the total plasma ion population. This alpha-population fraction is comparable to that expected to be reached in the high-temperature regimes of the CIT and the ITER. The world's first experimental study of alpha-particle plasma physics, in TFTR, is therefore expected to be of great scientific relevance and value.

The use of $D-He^3$ plasmas has been common in tokamak experiments to date (PLT, TFTR, JET), but since this fuel burns more slowly, its energetic reaction products form a smaller population of the plasma ions and give rise to weaker and less relevant plasma-physics effects. Alpha-particle plasma physics is sufficiently complex and imperfectly understood so that, if one wishes to rule out future surprises, there is no reliable substitute for the study of "real" alpha-particle plasmas.

7. Professor Bruno Coppi of MIT states in his testimony at today's hearing that "The possibility of the so-called 'breakeven', Q = 1, condition in TFTR looks very uncertain at this time." Do you agree or disagree with this assessment?

If D-T fuel had been introduced into last year's world-record-setting TFTR plasmas, they would already have yielded Q-values in the range 0.20-0.25. This summer, we hope to reach Q-values that are several times higher. Empirical formulas have turned out to be fairly reliable in setting lower bounds to tokamak performance: in the case of TFTR, the predicted lower bound for the Q-value is 0.5. Since TFTR has demonstrated the ability to operate in a powerful enhanced-confinement regime (the "supershot" regime), we should do considerably better than the lower bound, and expect to reach our Q = 1.0 goal.

Higher Q-values tend to be positively correlated with larger alpha-population fractions, but if the issue is whether important results will be obtained on alpha-particle plasma physics, it may be more useful to take direct note of alpha-particle parameters, as in the answer to question 6.

8. Professor Bruno Coppi of MIT makes reference in his prepared testimony to "the looming doubt that the considerable quantities of tritium needed [for the CIT] may not prove acceptable to the residents of the Princeton neighborhood". Is there any basis for such doubt?

There seems to be no basis for "looming doubt." Up to the present time, the Laboratory has received no expressions of concern from either officials or residents of the Princeton area. On the contrary, there have been numerous expressions of local pride in Princeton's leadership of world fusion research.

The Laboratory has been engaged in fusion research on the University's 360-acre Forrestal Campus since 1951. For the past thirty years, the research performed here has been unclassified and the facilities are currently being seen by five to seven thousand visitors each year. These visitors are of all ages, from school children to senior citizens. They include neighbors, business and community groups, scientists and laymen, and state and township officials from the surrounding area.

Beginning in 1975, plans for the use of tritium in TFTR were widely reviewed. These plans have been presented during the intervening 13 years to all who tour and visit the Laboratory, at open houses to which the public is invited, and in speeches to local groups. In the course of TFTR tours and open houses, Laboratory personnel make a point of discussing the nature of tritium and showing the tritium-handling and containment facilities (which will be re-used in CIT). Moreover, Laboratory information bulletins and brochures, widely distributed to the public and news media, describe in detail the use and handling of tritium.

The PPPL site is located inside a "greenbelt", protected by local zoning. The nearest corporate office buildings are about 500 meters away. The nearest residences are about a kilometer away. Extensive TFTR and CIT environmental analyses show that, even for the maximum credible accident involving a major earthquake, the impact would be insignificant and would fall well within all regulatory guidelines.

A formal technical Environmental Assessment for the CIT project is currently being carried out for the DOE by INEL. Next winter, we plan to discuss the CIT Environmental Assessment with state and township officials from the surrounding area, University officials, and neighbors of the Laboratory. We expect to present a thoroughly documented review of all CIT environmental issues.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF PHYSICS
CAMBRIDGE, MASSACHUSETTS 02139

22 April 1988

Ms. Lottie McCaw-Daniels
Subcommittee Office
B-374 Rayburn House Office Building
Washington, D.C. 20515

Dear Ms. McCaw-Daniels:

Referring to the letter of 5 April 1988 of the Hon. M. Lloyd, I am sending along my response for the record to the questions that had been forwarded to me with the same letter.

Thank you for your consideration.

Sincerely yours,

Bruno Coppi
Professor

BC/KSN

Enclosure

SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Hearing on the Department of Energy (DOE)
FY 1989 Authorization Request for the
Magnetic Fusion Energy Program

Responses to the questions of the Hon. S. Morrison
for the Record of the Hearing of March 30, 1988 by
Bruno Coppi, Professor, Massachusetts Institute of Technology

Dear Hon. Morrison

The costs that I have estimated in proposing an alternative scenario for the U.S. fusion program are indicated on the attached sheet. These include allocations to

i) boost the base program,

ii) construct the Compact Princeton Tokamak (C.P.T.),

iii) construct the Advanced Ignition Experiment (A.I.X.) at a nuclear site.

A

As proposed in my prepared written statement, the C.P.T. machine would follow TFTR at the Princeton Plasma Physics Laboratory, after this will have been adequately exploited. It should fit within the existing large power supply system and use the TFTR cell. The design of this machine that could be directed mainly to study the basic confinement properties of plasma columns carrying currents in the range 8-10 megampere, should be carried out expeditiously in order to avoid unacceptable activity gaps between the end of TFTR and the beginning of C.P.T. operation.

B

The A.I.X., as I envision it, would be strongly focused on attaining fusion burn and ignition conditions within a considerably shorter time than the presently proposed CIT program and with a higher degree of confidence. This goal is possible, given these relevant factors:

a) Constructing the machine at a certified nuclear site would guarantee that there are no obstacles to its operation with deuterium-tritium mixtures that involves the production of high fusion power levels, severe problems of activation, etc.

b) The selected site would be close to a node of a strong electrical grid, thus guaranteeing an economical and effective power supply and allowing a relatively wide selection of machine parameters.

c) This site would have pre-existing (fission) reactor containment buildings and all the relevant protection systems and expertise.

d) The need to add costly features to the machine (in order to make it a suitable experiment for a large and diversified scientific staff in the case where its deuterium-tritium operation may not be allowed by site and environment constraints) would not arise.

e) The machine design can be based on more compact dimensions and higher currents than the one presently proposed for CIT. Thus, it should allow A.I.X. to have higher confinement characteristics than CIT and require considerably less injected heating and related systems.

f) An upper bound to the dimensions of A.I.X. can be set by the study of the "Candor" experiment that we have carried out at MIT in cooperation with colleagues from a well-known multinational firm (Brown Boveri Corporation) that has a successful history in the construction of electrical machines

and key components of advanced fusion facilities (including JET in Europe and TFTR). One of the main objectives of Candor is to test the fusion burn conditions of deuterium-helium3 plasma mixtures. The annular (toroidal) plasma column of Candor has a major radius about 1.7 meters and an elongated plasma cross-section whose half-width is 0.65 meter and half height is 1.15 meter. The maximum plasma current that can be produced in this column is 18 megampere.

C

The Rapid Fusion Burner (R.F.B.) as I envision it, would be a machine with slightly better parameters than Alcator C-MOD, that would be relatively cheap to build (e.g. 100 M$), have only the essential diagnostics, and would have the objective to reach conditions somewhat better than breakeven but not ignition.

Before my testimony, I had the opportunity to hear the advice of a considerable number of friends and colleagues, as I tried to present a position representing a broad consensus. Two alternatives were discussed with them:

i) the R.F.B. followed by an ambitious A.I.X.;

ii) an A.I.X. carried out with a fast pace and in a spirit of austerity (therefore called "austere ignition experiment").

In the end, the second option carried more favor and in my oral presentation at these hearings, and in the final version of my written statement, I mentioned only the second option. Consequently, I have based my cost analyses on it.

However, my personal opinion is that, leaving scientific merit aside, the R.F.B. would, like the Alcator line of experiments, demonstrate clearly that systems involving fusion reacting plasmas could be built inexpensively and at a rapid pace. There is, in fact, a strong need to dispel the impression that meaningful enterprises dealing with fusion energy are remote in time and involve exhorbitant allocation of resources.

Yours Truly,

Bruno Coppi, Professor

TABLE I

PROGRESSIVE U.S. PROGRAM

(Large Scale Experiments and Base Program)

FY	88	89	90	91	92	93
AIX (M$)	16 (CIT)	53	79	93	95	102
TFTR (M$)	72	48	45	40	30	-
CPT (M$)	-	20	25	30	40	55
Base Program Addition (M$)	-	10	20	20	20	30
TOTAL (M$)	88	131	169	183	190	187
INCREMENT ABOVE DOE PLAN FOR LARGE SCALE (M$) EXPERIMENTS (TABLE II)	0	30	40	40	45	44
PRESENTLY PROJECTED TOTAL MFE BUDGET	335	360	375	392	409	426
PROPOSED PROGRESSIVE TOTAL MFE BUDGET	335	390	415	432	454	470
% INCREASE OVER PROJECTED BUDGET	0	8.3%	10.7%	10.2%	11.0%	10.4%
CUMULATIVE AIX	16 (CIT)	69	148	241	336	438
CUMULATIVE CPT	-	20	45	75	115	170

CAPTION OF TABLE I: PROGRESSIVE U.S. PROGRAM

Basis:

- The new Compact Princeton Torus (C.P.T.) replaces the TFTR device at Princeton's PPL.
- TFTR is assured its proper utilization by avoiding deuterium-tritium operation.
- The Advanced Ignition Experiment (A.I.X.) is constructed at the National Fusion Site.
- A.I.X. Startup is foreseen for 1993.
- A.I.X. deuterium-tritium experiments start in 1995/96.
- An addition is included to avoid irreparable decline of the base program.

Advantages:

- Locates A.I.X. at a long term nuclear site providing a future to the nuclear development of the U.S. fusion program.
- Provides an outstanding, "smart" physics facility for Princeton's PPL.
- Permits the reuse of the TFTR test cell, eliminates the probable environmental problems that a large deuterium-tritium burning experiment can cause in the Princeton area.
- Provides the base program a means to develop electron cyclotron heating for ignition grade plasmas. This being perceived as the best heating system yet available.
- Focuses the U.S. burning plasma program on accomplishing the A.I.X. experimental effort and strengthens the U.S. ability to attract international collaboration.
- Strengthens the university program and the cost effective development and utilization of experiments at U.S. universities and laboratories.
- Enables TFTR to run longer and concentrate on "smart" physics while also enabling the new CPT device to be fabricated and installed in place of TFTR, thereby minimizing the activity gap at PPPL.

TABLE II

PRESENT DOE PLAN

Large Scale Experiments

FY	88	89	90	91	92	93
CIT	16	27	46	64	95	101
TFTR	72	74	83	79	— 45	42
TOTAL (M$)	88	101	129	143	140	143
Cumulative for CIT	16	43	89	153	248	349

Basis:
- Carry out tritium experiments by the TFTR machine in 1991.
- Startup the CIT machine in 1996.
- Carry out deuterium-tritium burn experiments in CIT during 1998/99.
- Decline in basic plasma physics programs; no small initiatives.